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EDITED BY J. McKEEN CATTELL

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BAMBOO—A TAXONOMIC PROBLEM AND AN ECONOMIC OPPORTUNITY

By Professor F. A. McCLURE

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Some Aspects of the Technique of Bamboo Study

During the course of my work in the field of economic botany at Lingnan University, I have naturally become much interested in the bamboos, a group of plants of great importance in the agricultural and industrial economy of the Orient. About fourteen years ago I began to collect herbarium specimens in southern China, a region very rich both in variety and abundance of bamboos. The plants collected were submitted to specialists for identification. The determinations returned for the bamboos were particularly inconsistent and unsatisfactory. My subsequent experience has made clear to me the chief reasons for these faulty determinations.

The fragmentary nature of the specimens, including types, existing in the herbaria of the world; the inadequacy of the original descriptions, frequently combined with failure to cite type specimens; and the confusion of names which prevails in most of the literature, are conditions which, while not peculiar to this group, are particularly accentuated in respect to the bamboos. Again, the average collector has only the vaguest idea as to what constitutes an adequate herbarium specimen of a bamboo. And, naturally, he seldom recognizes the need of supplementing his specimens with notes about those parts of the plant which he can not conveniently take with him, beyond recording the height and diameter of the stems. The nature of the underground part (the rhizome) and the branching habit, both characters of great diagnostic value, are usually either neglected entirely, or referred to only in the most general, or even inaccurate, terms. The making of adequate specimens of bamboos, even under ideal conditions, requires special knowledge and special techniques just as certainly as does the making of usable specimens of fleshy fungi, for example, or of certain groups of algae.

In defense of the average collector, however, it should be said that, of necessity, he usually makes his specimens of bamboos during the course of routine collecting. Also, many of the bamboos are exceedingly variable in respect to certain characters or structures. And, most important of all, several very essential structures of the bamboo plant are available only at more or less remote intervals. Moreover, it rarely happens that all the structures are available in good condition for collecting at any one time. The leaves are, of course, present throughout the year as a rule, but, in response to various factors, they range widely in respect to size and shape.

CUTTING A BAMBOO SHOOT

THE EDIBLE BAMBOO SHOOTS PRODUCED AT CANTON IN SOUTHERN CHINA COME CHIEFLY FROM SYMPODIAL BAMBOOS (*Bambusa* AND *Dendrocalamus spp.*). THESE SHOOTS ARE USUALLY CUT BEFORE THEY HAVE EMERGED FROM THE HEAP OF EARTH THAT IS THROWN UP AROUND THE BASE OF EACH CLUMP. AFTER THEY HAVE EMERGED THEY RAPIDLY BECOME FIBROUS AND DEVELOP A BITTER PRINCIPLE WHICH MAKES THEM LESS PALATABLE.

The culm sheaths are structures of great diagnostic value in most species, yet these and the prophylls of the culm branches are usually available in good condition for collecting during only a short portion of each year. The flowers are produced only after a long period of development, at least in most of the bamboos about which anything is known in this respect. When it is remembered that the organs of reproduction are the traditional basis of all taxonomic work with plants, the indispensability of these structures for purposes of identification is at once apparent. This holds notably for the grasses generally, and especially for the bamboos, where the vegetative bases for identification have not yet been established.

Considerable study has been devoted, especially in Japan, to the problem of the causes of the flowering among the bamboos, and it is quite possible that a way may eventually be found to induce the plants to produce flowers at will. It appears that flowering occurs more commonly in certain species during years of drought; the drainage of an area occupied by bamboo has been known to precipitate flowering in the whole grove, and injury by fire is often followed by flowering in some species. But until some reliable method has been devised which will enable us at will to cause recalcitrant species to flower without dying, we shall have to work along other lines.

Rarity of flowering among the bamboos presents, then, a real problem to the taxonomist. But there is another aspect of the behavior of bamboos which is scarcely less disconcerting, and that is the suppression of their vegetative activity during the flowering period. It has been my observation that when a bamboo plant comes fully into the flowering or reproductive condition there is always a very definite suppression of its vegetative activity. That is, when flowers are produced, there is a more or less complete cessation of growth in the leaf, stem and branches. This is just what we would expect to find, knowing what we do about physiology. But it goes to different extremes in different species. In some (the monocarpic[1] ones) the vegetative activity is reduced to such a low ebb that the whole plant dies. This is the story that one has heard many times in connection with some of the bamboos of India. In other species, however, only the portion above the ground dies, while the rhizomes remain alive and gradually give rise to new plants. Recovery is very slow, however,

[1] The term monocarpic means "producing fruit but once and dying after fructification." This condition is the rule among herbaceous annuals, but it is a rare condition among woody plants.

since all the foliage and the stored food used in the production of the inflorescences are lost to the living part of the plants. The new stems and new foliage produced by the rhizomes are often very much reduced in size at first, and the stem-sheaths are scarcely typical for the first year or two after flowering has ceased.

In still other species, and more commonly than is generally supposed, the flowering normally does not result in the death of any part of the plant, but is accompanied, nevertheless, by a more or less complete cessation of all vegetative growth over a period of variable duration.

From what has just been pointed out about the suppression of vegetative growth during flowering, it will be apparent that certain structures will be poorly, if at all, represented in specimens collected from plants in flower. This applies particularly to the culm sheaths and all other parts which are quickly destroyed by weathering. Sometimes even the leaves are almost entirely absent, especially during the advanced stages of flowering, or they may be much reduced in size. If, however, specimens are taken from plants not in flower, as is often done in order to get a fuller representation of the vegetative parts, they are almost useless to the taxonomist, with the knowledge of the group in its present state.

A question may be raised as to the feasibility of taking flowering specimens from one plant and vegetative specimens from another of the same species. This has been attempted in many instances, but the procedure is fraught with very grave dangers, especially when the vegetative structures of the species in question have not already been associated unmistakably with flowers of the same species. An instance came to my attention some time ago where, in the description of what was taken for a single species, characters from four distinct species belonging to two genera were mixed together. This would not be done wilfully, of course, but it may easily happen as a result of associating specimens from different plants on the assumption that they belong to the same species.

BAMBOO SHOOTS IN THE MARKET

IN NORTHERN KWANGTUNG AND NORTHWARD THE MOST CONSPICUOUS BAMBOO SHOOTS ON THE MARKET ARE THOSE OF THE FAMILIAR *Phyllostachys edulis* (CARR.) HOUZ. LEH., A BAMBOO OF THE MONOPODIAL TYPE. IN WINTER THE SMALL, DORMANT SHOOTS ARE DUG UP AND MARKETED AS A DELICACY, BUT IN THE SPRINGTIME THESE ARE SOMETIMES ALLOWED TO REACH A HEIGHT OF SEVERAL FEET AND A WEIGHT OF TWENTY POUNDS OR MORE BEFORE THEY ARE CUT.

My preliminary work on the bamboos has convinced me that familiarity with the living plants is an essential preparation for sound taxonomic work on this group. To achieve this, one must be able to return again and again to the same plants to make notes and take specimens at the different stages of their development, in order to assemble a sufficiently complete array of the structures essential for purposes of identifica-

tion. This conviction led me to undertake to bring together, at Lingnan University, living plants of as many of the Chinese bamboos as will grow there.

The Lingnan University bamboo garden was started in 1920. To date about 550 introductions have been made, mostly from seven provinces of China but including a few from the Philippines and from Indo-China.[2] Every introduction is given a distinctive number, the plants are tagged, and their position in the garden is plotted. A record is kept of the source of each plant, the date of its introduction, its vernacular names, its uses, if any, and any other observations which seem pertinent. When a plant is first introduced, herbarium specimens are made of all the available structures, and a record of these is kept, along with the information just mentioned. It very often happens that neither culm sheaths nor flowers are available at first. As soon as these structures appear, they are collected and the fact is recorded. Then, as the picture of the plant represented by each introduction number becomes more and more complete, its identity becomes clearer and clearer, the identity

BRINGING BAMBOO SHOOTS FROM THE COUNTRY

IN APRIL, OVER NARROW, WINDING ROADS, FARMERS TREK TO TOWN BEARING BASKETS OF SUCCULENT BAMBOO SHOOTS.

[2] For plants of a number of species from Chekiang and Kiangsu provinces I am indebted to the Metropolitan Institute Museum at Nanking, through the coperation of the director, Dr. S. S. Chien, and Professor R. C. Ching, then botanist of that institution.

between different numbers becomes apparent, and finally, I have not only an adequate idea of the range of variation of each species, but I know its geographical distribution as well.

At first thought this seems like a long-time task. But one does not have to wait long for the results to begin coming in. Every year since the garden was started I have added numerous flowering specimens to the herbarium collection. Twenty-six of the plants were in a flowering condition when I left Canton in the summer of 1933. A subsequent report for last summer added another six numbers. A few have died in the act of flowering, while many have continued flowering over a period of years.

The central idea of this method, then, is to bring together a more complete picture of each species, embracing both the floral characters and the range of vegetative characters peculiar to each. Once accomplished, this will enable us to bridge the gap between any names which have been established in the literature on the basis of floral characters, on the one hand, and the plants as they grow, on the other. It will then be a relatively easy matter to identify bamboos in the vegetative state. Horticulturists have little difficulty in distinguishing the different kinds, once they are familiar with them. The difficulty is that no one is able, with certainty, to attach to many of them the correct scientific names. In the genus for which I have just finished a revision, I have had no trouble at all in finding good vegetative characters for distinguishing the species from each other.

Some Economic Aspects of Bamboo

The interest which bamboo has stimulated in the West grows largely out of the fact that this plant has, in many of its forms, played a very important part in the economic life of the people where it thrives, particularly in the Orient. Since very early times, no doubt, bamboo has supplied raw materials for food, clothing and shelter, as well as for many tools and utensils of everyday use. Even the essential writing materials, excepting ink, have been made of bamboo in China since the beginnings of historic times. Bamboo slips formed pages of the earliest books in China and, soon after the discovery of the art of making paper, bamboo became, and still remains,

A SHOP OF BAMBOO WARE

THIS DARK LITTLE SHOP IS FILLED TO OVERFLOWING WITH BAMBOO WARE OF EVERY DESCRIPTION —CHAIRS FOR YOUNG AND OLD, RICE BUCKETS, SIEVES AND TRAYS, CLOTHES POLES, RAKES.

so far as China is concerned, the paper pulp material *par excellence*. It is little wonder, then, that the civilization of the Orient has been spoken of as a bamboo civilization.

A list of the uses to which bamboos are put in the Orient to-day would be out of place here. One author recently published an incomplete list of some 200 such items. The compiling of such lists is a pursuit which has fascinated Western dwellers in the East since very early

THE PROTOTYPE OF OUR HOE DRILL IS FASHIONED FROM BAMBOO.

BAMBOO WATER WHEELS

IN EARLY SPRING SWOLLEN STREAMS ARE HARNESSED TO COMPLAINING BAMBOO WHEELS WHICH LIFT THEIR WATERS TO THE PADDY FIELDS.

times. A far more interesting and significant aspect of the subject, and one to which I have seen very little reference in the literature, is that of the special adaptabilities of many kinds of bamboos to particular uses. Speaking of the bamboo economy of any given area, some articles are so regularly made of a particular kind of bamboo that one can only conclude that there are definite because of the evenness of its grain and the lack of prominence of its nodes, yields the entire supply of split bamboo which comes to this country for making certain types of brooms and brushes. Still another species, which combines evenness of grain and smoothness of nodes with unparalleled toughness and resiliency, is the only species now used to any extent in all the world for mak-

WHERE BAMBOO ROPE IS MADE

BAMBOO ROPE OF VARIOUS KINDS IS WIDELY USED IN CHINA. THE TOWER HERE SHOWN IS USED FOR THE MAKING OF ONE SORT, WHICH IS BRAIDED FROM SLENDER STRIPS SPLIT FROM THE CULMS OF *Phyllostachys edulis*. IN THE VAT IN THE FOREGROUND, THE FINISHED PRODUCT IS BEING "CURED" IN LIME-WATER. THE LATTICED WALLS OF THE SHEDS AND THE PICKET FENCES ARE LIKEWISE MADE FROM SPLIT BAMBOO. TAKEN NEAR SHA-WU, KIANGSI.

correlations between the structure and mechanical properties of the stems of the different kinds, on the one hand, and the requirements of the respective purposes for which they are used, on the other. In southern China, for instance, a certain species of bamboo, because of the stiffness and ruggedness of its stems, is always used for scaffolding and the frames of mat sheds. Another species, ing split-bamboo fishing rods. Dr. Fairchild's article in a recent number of the *National Geographic Magazine* calls to mind an example from South America. Certain tribes of Indians there always make their blow guns, which are weapons of great accuracy, of a certain species of bamboo which is said to possess, among its distinctive characters, stems with one very long and very straight in-

ternode. Such examples could be multiplied indefinitely.

Speaking of bamboo in general, I will mention a few of the characteristics which give it certain advantages over similar materials, such as wood, rattan, etc. The physical properties of the stems, such as the hollow, cylindrical form, with diaphragms, make for relative lightness in proportion to strength. Its splitting qualities make for ease of working and economy of labor. The flexibility of slender strips of bamboo adapt it to the requirements of weaving and rope-making. The ease with which the stems may be warped by the use of heat contrasts bamboo favorably with other woods for many purposes. Finally, the natural stems lend themselves especially well to the making of various objects of rustic appeal. Then there is the ornamental value of the living plants, which makes them unique as horticultural subjects. The nutritive value of the young shoots, the forage value of the leaves, the high continuous productivity of bamboo per unit of land, once a plot is established, are points which suggest the promise of its winning eventually a place for itself among our economic cultures.

MAKING PAPER FROM BAMBOO

SOON AFTER THE DISCOVERY OF THE ART OF PAPERMAKING, BAMBOO BECAME, AND STILL REMAINS, SO FAR AS CHINA IS CONCERNED, THE PAPER PULP MATERIAL *par excellence*. THIS PICTURE SHOWS A RETTING VAT IN NORTHERN KWANGTUNG, FROM WHICH THE DIGESTED BAMBOO IS BEING REMOVED. THE MATERIAL IS NOW READY TO BE PULPED.

It will be necessary, however, to consider some of the potential obstacles to the rapid spread of the culture of bamboo in the United States. The unfamiliarity of our people with bamboo as a cultivated plant, with its cultural requirements and its potential usefulness in our scheme of things, must be overcome by gradual education. The present incompleteness of our knowledge concerning the kinds that will thrive in

BAMBOO AS A GARDEN WALL

As a living hedge, or a woven lattice, bamboo serves equally well to fence the dooryard garden of this Kwangtung farm.

AN INEXPENSIVE LANDING STAGE OF BAMBOO

which floats, solves admirably the wharf problem presented by a river which has an extremely variable level during the rainy months. West River, Kwangsi.

FOOTPATHS WIND THROUGH GROVES OF BAMBOO

Flecks of sunlight, and delicate shadows play upon the leaf-strewn floor of this inviting grove on the left. There are many such gardens within the walls of the city of Nanking, where this picture was taken. On the right, an ancient granite pathway winds its varied way through rugged bamboo country.

this country and their possible cultural range, must be corrected by continued experimentation. A cheaper supply of plants for propagation must be made available. The amount of hand labor required for harvesting and preparing the stems may be overcome in part by the development of new techniques. The competition of a cheap supply of stems from the Orient will be operative for an indefinite period, but there are certain advantages in having a convenient local supply of fresh stems.

There are also a number of obstacles which will hinder the spread of the use of bamboo in the United States. Its use for structural and other purposes would involve disharmonies with our established habits. The units are not as easily standardized or modified as sawn lumber may be. Bamboo doesn't take nails. The wood tends to split too easily, especially under conditions of extreme atmospheric dryness. Most articles made from it will not stand the rough usage that we are accustomed to giving things. Greater labor and ingenuity are required to use bamboo than to use sawn lumber or other familiar structural materials, since unfamiliar kinds of tools are required. The durability of bamboo out of doors, and particularly in contact with the soil, is not high. It is also very susceptible to the attacks of wood-boring beetles, and is ruined thereby.

However, in spite of these obstacles, and almost without benefit of organized propaganda, bamboo has already made a considerable place for itself here in the West. It is now being widely used as an ornamental plant in gardens, for fishing rods, as handles for collecting nets, as

garden and flower stakes, for vaulting poles, parts of airplane wings, and phonograph needles. And very probably bamboo will soon be recognized here as a soil-binding plant to prevent erosion, as a forage plant on eroding areas and more widely as a food plant. There remains a wide variety of incidental uses it will find when more extensively cultivated. In the making of such things as ornamental lattices, arbors and rustic furniture, temporary fences, fruit-picking poles, sticks for cotton swabs, tongue depressors, home-made toys, toothpicks, etc., its possibilities are practically unlimited.

These simple uses alone would make a more general culture of the bamboos

THE GRAY BLOOM ON GREEN BAMBOO CULMS

ADDS AN INDESCRIBABLE BEAUTY TO THIS FOREST CATHEDRAL.

A CORNER OF THE LINGNAN UNIVERSITY BAMBOO GARDEN
AT CANTON, IN SOUTHERN CHINA.

worth while. But there is another point at which I believe bamboo is going to make a really dramatic entry into our economic life, and this is as a paper pulp material. The process of pulping bamboo has already been patented in all the leading countries of the world, and a large paper mill has been established at Foochow, China, which is said to be capable of pulping even the mature stems. We are now importing considerable quantities of wood pulp from Canada and Europe. And it remains to be seen whether we shall import bamboo pulp from the Orient, as soon as their supply exceeds their demand, or whether we shall make an attempt to produce bamboo on a scale sufficiently large to support a pulp industry here. Perhaps we may witness both of these developments within the next few decades.

In conclusion, I wish to make a brief reference to the esthetic appeal and fascination of the bamboo. The beauty of the stately culms, their graceful swaying at the touch of gentle breezes, the soft-textured verdure of the foliage, are a never-failing source of inspiration and delight to the artist and to the poet. Not only has the bamboo motif greatly enriched the artistic heritage of the world, particularly that of the Oriental peoples, but the plant itself has been taken by the Chinese people as a symbol of uprightness, chivalry and gentleness. We in the West would do well to give ourselves more fully to the esthetic, as well as to the economic, appeal of the bamboo.[3]

[3] Grateful acknowledgment is here made of my obligation to the Division of Plant Exploration and Introduction, Bureau of Plant Industry, U. S. Department of Agriculture, for permission to make use of photographs taken by me while acting as agricultural explorer in China.

EXILED ELEPHANTS OF THE CHANNEL ISLANDS, CALIFORNIA

By Dr. CHESTER STOCK

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Introduction

Looking seaward from the strand at Santa Barbara, there may be discerned on a clear day one or more of the insular masses that skirt this portion of the coast-line of southern California. Extending from west to east the Santa Barbara or Channel Islands, as they are called, include San Miguel, Santa Rosa, Santa Cruz and Anacapa. The marine passages or channels which separate from each other the individual members of this group range in width from three to five miles and in depth from 17 to 110 fathoms. Between the islands and the mainland is the Santa Barbara channel which, opposite Santa Rosa, has a width of 29 miles and a maximum depth of 356 fathoms. In a southeasterly direction this channel becomes narrower and shallower. Opposite Anacapa its width is reduced to approximately 12 miles, while the depth here is for the most part less than 100 fathoms.

These islands rise from the submarine shelf and are now being gradually demolished by the ceaseless pounding of the Pacific, but agencies responsible for subaerial erosion are likewise assisting in the process of degradation. Considerable relief of the land surface is to be seen, particularly on Santa Cruz, the largest of the group, where elevations of 2,000 feet or more above sea-level are reached. The height of the land decreases on the islands lying westward of Santa Cruz and on Anacapa to the east.

Through the years much interest has been manifested in this insular group. In passing, mention may be made of only a few of the features that have served to attract students in historical and scientific research. The archeological materials which have been found there furnish an important record of the coast tribes of Indians of southern California. It is said that Cabrillo, the intrepid conquistador who discovered these aboriginals, is buried on San Miguel. To students of modern plants and animals and of their distribution in the Californian region, the flora and fauna of the Channel group offer much to engross attention. While there is no dearth of references to the geological features, the more general accounts and stray notes of earlier years have only recently given way to more detailed geological studies.

That the islands were once part of a peninsula, extending westward from what is now the mainland east of Anacapa, was advocated many years ago by Yates and more recently by Chaney and Mason.[2] An understanding of the geological history which this insular area has undergone is basic to a fuller appreciation of the biological features that characterize the region to-day. With notable changes in environmental conditions at least in part consequent to the partial subsidence of this ancient land-mass, not only was the character and distribution of the life of the area affected, but the present peculiarities are likewise dependent upon the time when this occurred. Thus the islands present a fascinating biogeological study, in many respects comparable to that en-

[1] Contribution No. 162.

[2] R. W. Chaney and H. L. Mason, Carnegie Inst. Wash. Publ. 415, Art. I, 1930.

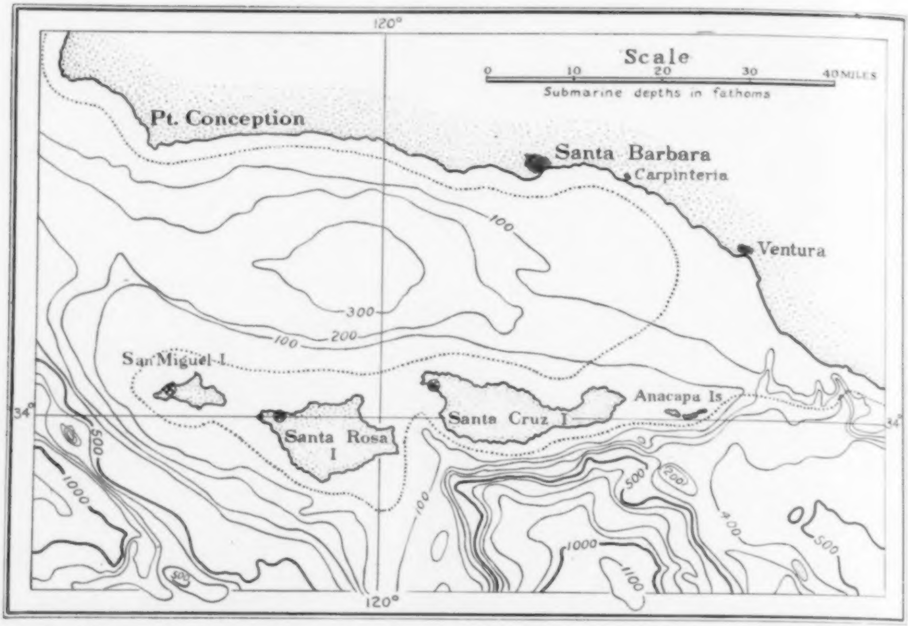


FIG. 1. MAP OF COASTAL PROVINCE OF SOUTHERN CALIFORNIA IN VICINITY OF SANTA BARBARA. LOCATION OF SOME OCCURRENCES OF FOSSIL ELEPHANTS ON CHANNEL ISLANDS SHOWN BY ×. DOTTED LINE INDICATES HYPOTHETICAL BORDER OF LAND DURING PLEISTOCENE TIME, AFTER CHANEY AND MASON.

countered on other continental islands of much larger size.

No more pertinent information indicating the lateness of the period of island formation can be furnished than that presented by a record of the former presence of elephants in this area. Aside from the importance of the occurrence of fossil mammals, however, the types themselves are of special interest because of their unique characteristics.

GEOLOGIC OCCURRENCE

Remains of extinct elephants are now known to occur on three of the Channel Islands, namely, on San Miguel, Santa Rosa and Santa Cruz (see Fig. 1). The first material was found on Santa Rosa more than sixty years ago, and this island has furnished by far the largest collection of fossil specimens representing these types. Similar material has been brought to light on San Miguel. In contrast to the rather numerous finds of elephant remains in Quaternary deposits of Santa Rosa, the presence of elephants on Santa Cruz is known thus far by only two fragmentary enamel plates of a cheek-tooth.

San Miguel: Although this island is wind-swept and shifting sand dunes mantle much of the area underlain by sediments of Tertiary and Quaternary age, the incision of the present land surface by ravines and gullies and the constant though gradual recession of the sea-cliffs develop exposures on which occasionally the weathered-out materials of fossil mammals have been discovered. Several tusks and cheek-teeth of elephants were found in a thin series of Quaternary alluvial deposits lying beneath a table-like surface and exposed in the sides of gullies near the northwest end of San Miguel. Scattered proboscidean teeth have been found from time

FIG. 2. GEOLOGIC MAP OF SANTA ROSA ISLAND; AFTER W. S. W. KEW. NOTE OCCURRENCE OF QUATERNARY BEDS AT NORTHWEST END OF ISLAND. DEPOSITS OF THIS AGE AND CONTAINING ELEPHANT REMAINS ARE NOW KNOWN TO EXTEND CONSIDERABLY FARTHER TO THE EAST.

to time elsewhere on this island. Among the fossil materials are specimens which clearly point to the fact that the San Miguel elephants are among the largest types to be obtained in the insular region.

Santa Rosa: Deposits containing remains of elephants occur at several localities, but the more significant discoveries of such materials have been made in Quaternary sediments forming a mantle over Tertiary rocks near the northwest end of this island (see Fig. 2). Excellent exposures of the fossil-bearing strata are extensively developed in the sea-cliffs and along the ravines or cañadas. Moreover, opportunity is afforded in this region to determine the relationship of these Quaternary deposits to the geologic events that transpired prior to and following their accumulation.

If we turn for a moment to the geologic events subsequent to the tilting of the Miocene marine sandstones and shales also exposed here, we note the clear evidence of at least three marine transgressions across these older rocks as well as several stages of uplift. The highest of the cut surfaces or marine terraces observed in this area has now an elevation of approximately 400 feet. The lowest cut surface, above the present strand, truncating the tilted Miocene rocks, has an elevation of approximately 10 feet. Between the two is another bench, bordered in front by a cliff which descends to the surface of the lowest terrace. The rear border of this intermediate bench is likewise formed by a cliff, whose position corresponds with that of the front of the upper terrace. The recorded diastrophic events and successive incursions of the sea are not unlike those which have been recognized on the mainland, along the Santa Monica coast and elsewhere. Resting on the lowest and intermediate benches are the Quaternary sediments which consist of gravels, sands or sandstones, coquina-

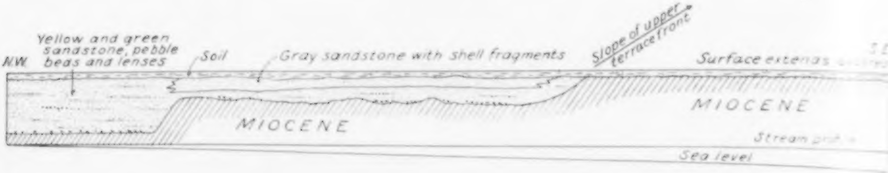


FIG. 3. GENERALIZED GEOLOGIC SECTION SHOWING RELATIONSHIPS OF THE QUATERNARY, ELEPHANT-BEARING BEDS TO THE UNDERLYING TERTIARY MARINE SEDIMENTS. NOTE THE LOWER AND INTERMEDIATE BENCHES (MARINE-CUT TERRACES) TRUNCATING THE TILTED MIOCENE ROCKS. DATA AND DIAGRAM FURNISHED BY F. D. BODE.

like deposits, and alluvial detritus. A generalized geologic section showing the relationships on the west fork of the Cañada Tecolote is given in Figure 3. Elephant remains are found in these deposits from near the base to within a short distance of the surface.

The Quaternary strata have a thickness of 40 feet or more and are capped by a remarkably even surface having an elevation of 75 feet or less above sea-level. This surface is well defined and parallels the coast for a considerable distance (Fig. 4). Unlike the upper terrace, the lower surface is not wave-cut but appears to have been formed as an aggraded surface by deposition of sediments in stream courses and in areas adjacent to the ocean strand after the streams, flowing from the more central portions of the land-mass to the sea, had become graded with respect to sea-level.

FIG. 4. VIEW LOOKING WEST ALONG NORTH FRONT OF SANTA ROSA ISLAND FROM ENTRANCE TO CAÑADA CORRAL

THE UPPER MARINE TERRACE AND THE LOWER SURFACE ARE CLEARLY SHOWN. EXTENSIVE EXPOSURES OF THE ELEPHANT-BEARING DEPOSITS MAY BE SEEN IN THE SEA-CLIFFS BELOW THE LATTER. SAN MIGUEL ISLAND SHOWN ON RIGHT HORIZON.

It is important to recognize, however, that since this has occurred, uplift of the land-mass has brought this surface to its present position and a new cycle of degradation and deposition has been initiated.

In the light of known information concerning the diastrophic history of the coastal province of southern California, it is reasonable to assume that the marine incursions and changes in elevation on Santa Rosa Island form part of a long and important series of geologic events featuring this province during glacial and postglacial time. Whether or not Pleistocene or Ice Age elephants had gained access to this portion of the ancient peninsula prior to the transgression of the sea, responsible for the cutting of the upper (400 foot) terrace, remains to be determined, but the advent of these creatures prior to that time may well have been the case. It appears probable that if the elephants had been present the period of their isolation occurred at that time. On the other hand, the length of time which has elapsed since mammoths were present in the region may be measured principally in terms of earth movements which have brought the elephant-bearing strata to their present position and in terms of erosion.

At one locality at the base of the Quaternary strata occurred a whale vertebra and it may be presumed from this and from similar material found elsewhere that the cycle of deposition which brought about the accumulation of these sediments was initiated at least in some places under marine or estuarine conditions. The areal distribution of the Quaternary deposits and their abrupt termination along the present shore-line make it appear quite probable that the elephant-bearing beds extended originally for some distance northward, that is to say into the region now occupied

FIG. 5. EXCAVATING AN ELEPHANT TUSK

IN QUATERNARY DEPOSITS EXPOSED IN THE CAÑADA GALLION. SANTA ROSA ISLAND, CALIFORNIA.

by the Santa Barbara channel. In the sequence of events that transpired during the period when elephants were living at this locality one may envisage a stage when shallow bays or arms of a large embayment fringed this section of the insular area, into which emptied streams that drained the hinterland. Under these conditions opportunities

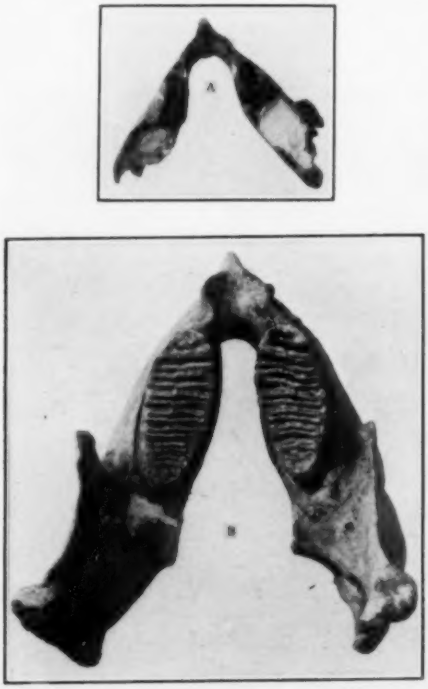


FIG. 6. *ARCHIDISKODON EXILIS* (STOCK AND FURLONG)
LOWER JAWS, VIEWED FROM ABOVE; *a*, FOETAL SPECIMEN, No. 172; *b*, PART OF TYPE SPECIMEN, No. 14, APPROX. × ⅓. CALIF. INST. TECH. COLL. QUATERNARY DEPOSITS, SANTA ROSA ISLAND, CALIFORNIA.

permitted an accumulation of sedimentary beds and an entombment of organic remains, similar to those which prevail to-day on a minor scale along this portion of the island coast.

Santa Cruz: Quaternary deposits occurring at the southwestern end of this island have been described recently by Chaney and Mason. These beds are particularly noteworthy because of a fossil plant record which they contain. Among the materials now known are logs of Douglas fir, representing a tree which in its native state is not known to range at the present time south of the coastal belt of northwestern California.

In the course of archeological field investigations conducted several years ago at the western end of Santa Cruz, two fragmentary enamel plates of an elephant tooth were found lying on an eroded surface of Quaternary deposits exposed along the ocean front, a mile or more away from the locality where the plant specimens occurred. A search conducted later for additional remains proved unsuccessful. In this area are a number of kitchen midden accumulations lying on Quaternary beds, and present erosion has washed and scattered some of the detritus from the middens over the older sediments. While the presence of tooth-fragments may well indicate the former existence of elephants on Santa Cruz, particularly in view of the occurrence of these animals on the more westerly situated islands, the possibility that the fragments were brought by the aboriginals to this island from Santa Rosa or San Miguel can not be wholly ignored. It is quite possible that such materials were seen and on occasion were picked up by the native inhabitants and may have been transported from one island to another with other articles of barter.

Fossil Elephants

It is clear from the occurrence of proboscidean remains on islands of the Channel Island group that elephants must have gained access to this region, prior to its present insular state, by way of some land-bridge or connection with the mainland. The alternate hypothesis, that mammoths reached the islands by crossing marine barriers more or less comparable to those that exist in the region to-day, can not be countenanced seriously in view of the formidable char-

acter of these barriers and in view also of the presumed habits of the extinct elephants as inferred from those of their living relatives. Frequent finds of fossil elephants made in Quaternary deposits of the Californian area clearly demonstrate that these mammals were well represented on the mainland and that they enjoyed a wide distribution over this western territory. Indeed, this may be said for much of the North American continental area during certain stages of the Ice Age. It is therefore quite permissible to assume that when the land in what is now the western end of the Santa Monica Mountains and contiguous area continued farther out to sea, elephants found no hindrance to an extension of range into this region.

It is important also to recognize that since the proboscideans, whose remains are found on the islands, had reached a stage of development in their evolutionary history characteristic of Quaternary time, their entrance into this land-area must have occurred during the geologic epoch immediately preceding the Recent. The length of time which elapsed during the Pleistocene or Ice Age is doubtless to be measured in terms of hundreds of thousands of years, but as yet the first appearance of elephants on that ancient land-mass can not be definitely fixed in Pleistocene chronology. As suggested under the discussion of occurrence, this event may have taken place prior to a time of considerable inundation of the land as represented perhaps by the stage of cutting of the terrace which now stands at an elevation of 400 feet or more above sea-level along the northern border of Santa Rosa Island.

At least two species of elephants, namely, the imperial and columbian mammoths, have been recognized in the fossil record of the mainland of California. As a matter of fact, these species are now included under separate genera,

FIG. 7. SEA-CLIFF SECTION OF ELEPHANT-BEARING BEDS
NEAR MOUTH OF CAÑADA CORRAL SHOWING POSITION OF ELEPHANT SKULL BEFORE EXCAVATION. SANTA ROSA ISLAND, CALIFORNIA.

Archidiskodon imperator and *Parelephas columbi*. When complete skulls and skeletons are lacking, specific identification of remains has been based upon recognition of structural characters in individual cheek-teeth. However satisfactory or unsatisfactory this may be, the island elephants show some resemblance in the characters of the cheek-teeth to the mainland types. In at least one particular, aside from the dental characters, they differ from the latter and in consequence were recognized by Stock and Furlong[3] as belonging to a distinct species, *Elephas* (*Archidiskodon*) *exilis*. Whether or not more than one species of elephant is present among the island forms remains to be definitely determined. In this connection, it should be recognized that an interesting and perhaps significant difference may exist between those forms on Santa Rosa and the types on San Miguel.

[3] C. Stock and E. L. Furlong, *Science*, n. s., 68: 140–141, 1928.

Numerous cheek-teeth and tusks, fragmentary jaws and skeletal elements comprise the bulk of the collections obtained on Santa Rosa. Individuals of all ages are preserved, from an unborn type to fully grown adults. The youngest specimen, evidently belonging to a foetus, is represented by a lower jaw (Fig. 6) in which the enamel plates had not firmly consolidated to form the lower cheek-teeth and had not erupted through the gums. One fairly complete skull represents an adult individual and furnishes valuable information as to the specific characters of the island elephants. When found in Quaternary strata, exposed in the sea-cliff near the mouth of the Cañada Corral, only the weathered cranial portion was visible (Fig. 7). Excavation revealed the rest of the skull and upper tusks (Fig. 8)

FIG. 8. SAME LOCALITY AS THAT SHOWN IN FIG. 7
EXCAVATION HAS EXPOSED SKULL AND TUSKS OF TYPE SPECIMEN OF *Archidiskodon exilis* (STOCK AND FURLONG). SANTA ROSA ISLAND, CALIFORNIA.

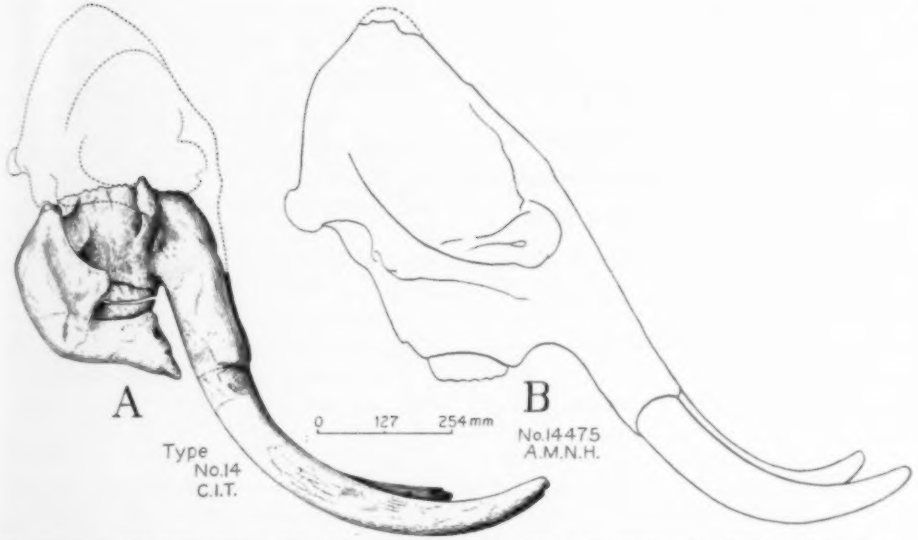


Fig. 9. Comparative views (to same scale) of (A) type specimen of *Archidiskodon exilis* (Stock and Furlong) from the Quaternary of Santa Rosa Island, California, and (B) young adult of *Archidiskodon imperator* (Leidy) from the Pleistocene of Texas. Figure 9B reversed, after Osborn.

with the lower jaw in position below the palate. Illustrations of this specimen and of a young adult skull of the imperial mammoth (*Archidiskodon imperator*), drawn to the same scale, are shown in Figure 9.

Comparison of fossil remains of elephants found on Santa Rosa with comparable materials occurring on the mainland establishes clearly the fact that the island forms were smaller in stature than their relatives of the mainland. Considerable variation in size exists among the island types, but the difference in stature between island and mainland forms remains a notable feature. This difference is apparent in Figure 9 and is strikingly demonstrated likewise in a comparison of limb elements (see Fig. 10). While the elephants of the mainland ranged in height from approximately 10½ feet tc 13½ feet as measured at the shoulders, those of the islands presumably never exceeded 8 or 9 feet in height and the smaller individuals were probably no taller than 6 feet. Thus, the smaller size of these elephants presents a character wherein they resemble the fossil or subfossil, dwarfed elephants described from the Maltese Islands of the Mediterranean. The diminution in size, however, has not been carried so far in the Channel Island elephants as in the Maltese species.

When an attempt is made to account for this distinguishing feature among the island types, two alternatives present themselves. Either the elephants that originally penetrated this ancient land-area were a race of small individuals, or the mammoths now found fossil on the islands exhibit a smaller size, in contrast to mainland forms, as a direct or indirect result of their insular isolation. As yet, no information is at hand to indicate that a species of elephant with the characters of the island forms was in existence during Quaternary time along the littoral of the southern California mainland.

On the other hand, when the difference between the island and mainland

FIG. 10. DIAGRAM SHOWING COMPARATIVE LENGTHS OF HUMERUS IN PLEISTOCENE AND RECENT ELEPHANTS. THE NUMBERS REFER TO THE LENGTHS OF THE HUMERUS IN MILLIMETERS. NOTE THE SHORTNESS OF THE BONE IN THE ISLAND SPECIES, *Archidiskodon exilis* (STOCK AND FURLONG).

forms is coupled with the fact that oscillations of the land occurred in late geologic time in the region now occupied by the Channel Islands, one may be led to assume that restricted terrain, limited environmental conditions and food-supply and inbreeding have been influential in establishing the specific characters possessed by the island elephants. Certain it is that these conditions, and perhaps others, would arise, were island isolation the final stage in the history of a group of mammals whose former range extended over a much larger and continuous land-area.

As mentioned before, the elephants of San Miguel are among the largest types to be recorded from the island region. Tusks of these forms have been found which measure 5 feet in length and 6 inches in diameter at the base. While some of the fossil materials on Santa Rosa likewise indicate the former presence of relatively large individuals, it is possible that the average size of the San Miguel elephants was larger than that of the Santa Rosa types. Were this ultimately established to be the case, on the basis of a comparison with more extensive collections than are now available from San Miguel, it is interesting to speculate whether the difference may not have been the result of an earlier extinction of elephants on the smaller of the two islands.

CONCLUSIONS

The occurrence of extinct elephant remains in deposits of Quaternary age on islands of the Channel Island group points to the former presence of a land-connection between the insular area and the mainland. The fact that these proboscideans were in a Quaternary stage of evolution when they lived on the ancient land-mass emphasizes the relative recency of the geologic events that brought about the insular conditions.

No small elephants with the characters of those found on the islands have been recognized thus far in Quaternary deposits of the mainland of California. It is fair to suppose, especially in the light of the later geologic history of the island area, that the characteristic size of the island elephants results from insular isolation. Further, the difference in stature between island and mainland forms may be an index of the length of time which elapsed during the period of exile.

MARRIAGE AND SEX CUSTOMS OF THE WESTERN ESKIMOS

By CLARK M. GARBER

FORMERLY PROFESSOR OF BIOLOGICAL SCIENCES AT CAPITAL UNIVERSITY AND SUPERINTENDENT OF ESKIMO AND INDIAN SCHOOLS IN ALASKA FOR THE UNITED STATES BUREAU OF EDUCATION

AN intelligent discussion of the marriage and sex customs and practises of a primitive people presupposes the author to have lived with them for a comparatively long period and to have made a systematic and organized study of their culture. One is not given the opportunity to study the intimate life habits and customs of a people by brief contacts with them. A perspicuous knowledge of the Eskimos' private life can be acquired only by living intimately with them, and particularly through the enjoyment of their utmost confidence. The scientist or student who would learn the things which lie within the secret lives of the Eskimos must be prepared to isolate himself from the civilized world, and for a period of time virtually become an Eskimo in his life habits. A few scientists and students—Nelson, Petrof, Rassmussen, Jenness, Stefansson, etc.—have been willing to suffer such isolation and hardships in order that they might contribute accurate information to the world's knowledge of Eskimo culture, while others have been content with cursory investigations. The cursory investigation can lead only to distorted ideas, exaggerations and false information. Unfortunately, much of this misinformation finds its way into print for the reading public to consume, and thereby false conceptions concerning the Eskimo people are broadcast.

The short-time investigator is unknowingly apt to find the Eskimos prepared to give him just the information he wants. They readily sense what is desired and often purposely deceive those who would learn their secrets. Furthermore, the Eskimo is prone to put only his good side forward. In his legends of conflict with the white man his side is always superior in arms, skill, strategy and shrewdness. His answers to interrogators are made to fit as near as possible to his interpretation of the white man's ethics. But there can be no doubt that a conscientious and intensive study of the Eskimo in his living state will yield a more lucid conception of his culture than will be gained by a lifetime study of museum specimens, especially when such study is the result of a comparatively long and intimate life with them. A picture of the Eskimos' past culture may be drawn from archeological specimens taken from reputed ancient village sites, and may also be drawn from their present culture and folk-tales handed down through innumerable generations. But if our store of knowledge of Eskimo life, past and present, is to approach a reasonable degree of accuracy it must be built upon an exhaustive study of both these sources of information.

The customs and traditions of a primitive people vary according to locality, clan, tribe, village or dialect, and thus we find it among the western Eskimos. This variation is not as distinct to-day as it was prior to their contact with the white man. Demarcation lines between tribes or clans have melted away, because of the white man's efforts to Americanize the Eskimos. It is therefore very difficult to determine where the Innuits, Kuskokwagamutes, Nunivagamutes, Malmutes or Unalits begin or end, and unless the investigator is familiar with Eskimo philology he may

find himself at a loss to differentiate them.

The Inception of Sex Knowledge

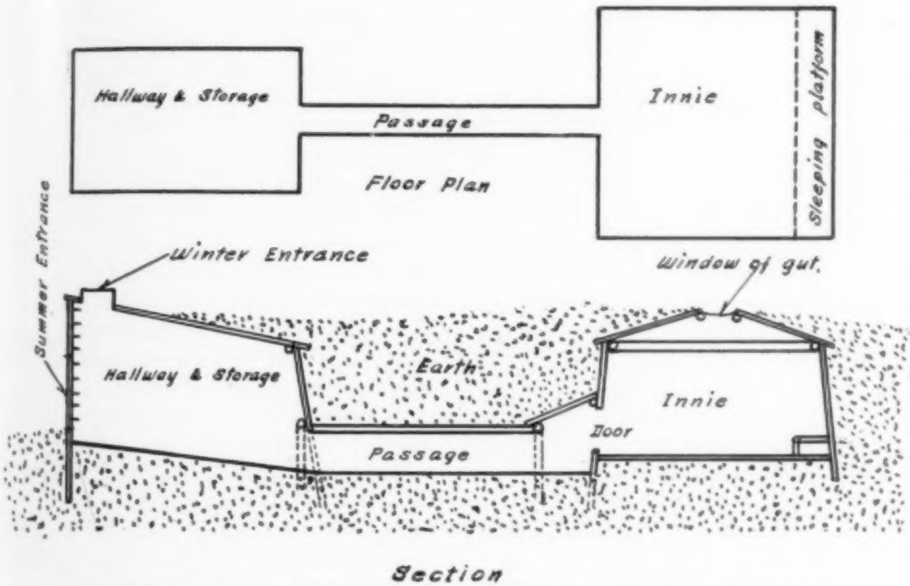
To the reader it may be an astounding fact that the Eskimo children learn of sex matters as soon as they are able to form ideas. To them it is not a matter of parental teaching in any form. It is a matter of observation, because they witness family functions being conducted right before their eyes. There is no effort on the part of the parents to conceal such matters from the children. The Eskimos' permanent home is an underground structure called an Innie.[1] The floor area of the actual living quarters does not average more than two hundred square feet. In this small room may often be found living a father and mother, three to six children, possibly an older son or daughter who is married, and likely the grandparents. Under such crowded conditions in such a small place the children, from the youngest up, witness the performance of all family functions. By their nature Eskimo children are great imitators. They attempt to mimic everything they see done. Hence their early and natural attempt at participation in sex relations, particularly during their play at housekeeping. I have witnessed Eskimo children at play during which the mimicry of the sex act of their parents was just as important a part as the sewing, tanning, caring for the imaginary children, etc. The dolls of the Eskimo children are in most cases fashioned in the form of the complete nude of either sex, *i.e.*, they are not asexual. At the age of ten to twelve years the Eskimo boys and girls arrive at puberty. No sooner is this in evidence than the parents are anxious to marry them off so that they may enjoy further means of support during their declining years. This is particularly true of the girls, since they do not contribute to the support of the family. Among the Innuits it was not uncommon to mark a girl by tattoo lines as an indication that she had attained the marriageable age. In the region of the Yukon and Kuskokwim Rivers Delta the girl's father would pass the news among the young hunters that his daughter had arrived at her first catamenia and could therefore be married any time. In this same section I found several instances in which the girl's mother, by physical manipulation, prepared her small daughter for the process of copulation in order that complete satisfaction might be assured the prospective husband, who was a skilled young hunter having the marks of an excellent provider. However, mating does not always take place immediately after puberty. It is often postponed until much later, and I have known both girls and boys to be between twenty-five and thirty years old before they were permanently married.

In the translation and compiling of a volume of folk-tales of the western Eskimos[2] I have found a large per cent. of their stories and legends based upon sex, mating, propagation and birth. These stories are often told to children as bedtime stories; hence they early in life become sex-minded. For instance, in their stories, "The Daughter of a Witch Doctor" and "How the Yutes first came to Nuviakamute," direct and literal reference is made to cohabitation, childbirth and sexual indulgences. In their folk-tales they talk about sex exploits as familiarly as we tell our children about Red Riding Hood.

Child betrothals among the western Eskimos are not uncommon, although it has never developed into a general practise or custom. Usually such betrothals take place between parents who are particularly affluent, and wish their good

[1] *Innie:* The permanent home of the western Eskimos built under ground.

[2] "Folk Tales of the Western Eskimos," by Clark M. Garber. Not yet published.



FLOOR PLAN AND SECTION OF A BERING STRAIT TYPE INNIE

IN ANOTHER TYPE OF INNIE FREQUENTLY CONSTRUCTED BY THE WESTERN ESKIMOS THE DOOR TO THE INNER CHAMBER CONSISTED IN AN OPENING IN THE CENTER OF THE FLOOR. THIS OPENING WAS CONNECTED BY A DEEPER TUNNEL RUNNING DIRECTLY UNDER THE INNIE FROM THE HALLWAY.

fortune to continue with their children. Or the parents of the betrothed children may be especially good friends and desire to honor each other through the betrothal and subsequent marriage of their children. In the case of a child betrothal the actual marriage does not take place until the girl and boy have reached the age of puberty or later. In the meantime they are not permitted any sexual freedom between them, but each may have such indulgences with others. Nelson says,[3] "From the lower Yukon to the Kuskokwim child betrothals are common and may occur in two ways. In such cases the girl is frequently not over four or five years old. Sometimes such arrangements are made by a couple to take effect when the first girl is born. In these child marriages when the girl reaches puberty both she and her husband are considered unclean, and neither of them is permitted to take part in any work for a month, at the end of which period the young husband takes presents to the kashim and distributes them. After this he enjoys the rights of other heads of families."

PROMISCUITY

The freedom of sex relations among the western Eskimos and particularly among their unmarried youth can not, by any reason or theory, be laid at the door of licentiousness. Such freedom of sex can be interpreted only as a biological development which has for its purpose the propagation and perpetuation of the race. I regret to say that the lewd side of Eskimo life developed as a result of their contacts with the white man. Murdock says of the Point Barrow Eskimos,[4] "promiscuous sexual intercourse between married and unmar-

[3] Nelson, Eighteenth Annual Report, Bureau of American Ethnology, pp. 291, 292.

[4] Murdock, Annual Report, Bureau of American Ethnology, No. 9, p. 419.

ried people, or even among children, appears to be looked upon simply as a matter for amusement." With due deference to Mr. Murdock's investigations, I fear that he was somewhat deceived by appearances. The amusement side of sexual freedom among the western Eskimos did not develop until they learned from unscrupulous whites about the sensuous pleasures they might derive from the abuse of a natural process which, up to that time, functioned in a natural way. Of course we can not believe that the Eskimo, even in his most primitive state, is ignorant of sex gratification. That would be both unreasonable and unnatural, but the conversion of their natural sex relations to a state of licentiousness was inaugurated by foreign influence. We learn by life in an Eskimo colony that the abuse of the practise of sex freedom was frowned upon. Farther on in his report, in his criticism of Mr. Simpson's writing, Mr. Murdock says,[5] "since the immorality of these people among themselves, as we witnessed it, seems too purely animal and natural to be of recent growth or the result of foreign influence." The proper interpretation of this statement hinges on the word immorality in the sense in which Mr. Murdock applies it to the practise of sex freedom. After many years of intimate life with the western Eskimos it is impossible for me to interpret their sexual freedom in any sense of moral turpitude, because it is purely biological. It is fairly certain that they would not talk to their children in unrestrained terms regarding sex matters if they knew it to be contrary to custom and tradition. It is around custom and tradition that their sense of right and wrong is built. What appeared to Mr. Murdock to be amusement in connection with their sexual freedom was naught but their amusement at the white man's inquisitiveness.

[5] Murdock, Ninth Annual Report, Bureau of American Ethnology, p. 420.

The birth of a child out of wedlock is considered very improper, and usually means a severe scolding from the parents. But a girl who has given birth prenuptially is by no means disbarred as a candidate for marriage. In fact, her fecundity is thereby proven, and since children represent the criterion of a happy marriage she has no particular difficulty in finding a husband. It is in reality a greater crime for a girl to be barren than to have children out of wedlock. A girl who is barren is wanted by no man. She may become the second or third wife of a man, but because of her inability to bear children she will have all the menial tasks about the household to perform. In a later paragraph—"Influence of the Whites"—it will be found that prenuptial childbirth is often favored, especially when the father is a white man.

Promiscuous sexual intercourse among the youth of a primitive Eskimo village seldom, if ever, becomes a licentious matter to the extent of producing prostitutes. Prostitution found among the Eskimo girls is not a natural result of sex freedom. It is the result of the white man's perfidy. In all their legends, stories and folk-tales we find not a single reference to any condition or situation which might be interpreted as prostitution. Widows are more apt to become prostitutes than unmarried women. A widow who has already borne a family often becomes the common property of the men of the colony. The young men prefer her services to the services of the young girls. This apparent prostitution does not bar the widow from subsequent marriage, and when such marriage does occur she again becomes chaste. Any children she may have borne during her widowhood as a result of her prostitution are given away to childless couples, who are highly pleased to get them. In my inquiries into such cases of prostitution I have found that the widow does not so behave

of her own choice. Custom and tradition wield a powerful influence in the matter, so that she is more often forced into prostitution against her desire than by her desire.

Marriage Customs and Ceremonies

The primitive Eskimo has never developed a complex ceremony by which husband and wife are bound in wedlock. His mating is almost as simple as the mating of wild animals. His marriage is based on the economics of his existence. In the first place a wife who is a good housekeeper, skilled in making of fur garments and footwear, and efficient in the butchering and preparation of meats and skins, is most desirable. But the predominant factor in Eskimo mating is the wife's ability and willingness to bear children. Every young hunter in contemplating marriage considers this factor first. This is partly due to nature's scheme for propagation and perpetuation of the race, and partly due to tradition and custom. The result is a probationary marriage. The young hunter looks upon all the likely girls in the colony and observes them at their daily tasks. When he has decided upon the girl whom he believes will make him a good wife he approaches her father or guardian to arrange the matter. The prospective father-in-law, who is usually glad to be rid of his daughters, may agree to the young man's proposal, but exacts from him a promise to hunt and fish and turn the proceeds over as his consideration in the agreement. This, however, does not consummate the marriage relation. In the Yukon and Kuskokwim Delta region the young hunter takes the girl to his camp and lives with her in a state of trial marriage for a year. If children are begotten, proving the girl's fecundity, the marriage is thereby consummated. But if the girl fail to become pregnant with child she is returned to her parents, and the young hunter is free to select another. In the Bering Strait region and on St. Lawrence Island the period of probationary marriage may endure for three years, during which time the girl remains with her parents and the young hunter must turn over his entire catch to his prospective father-in-law. Co-

Photo by A. B. Martin

A TYPICAL ESKIMO WOMAN

OF THE KUSKOKWIM RIVER DELTA REGION. THE ORNAMENTS WORN ACROSS THE BREAST OF THE PARKA ARE TWO BRASS ALARM CLOCK WHEELS, AND BETWEEN THEM A RUSSIAN BELL. APPROXIMATE AGE 22 YEARS.

habitation of the young hunter and his trial wife is permitted during this period, and if the relation does not prove satisfactory it may, without ceremony, be broken by either party. In such cases the trial husband is the loser, because he will have hunted and fished for the girl's

parents and receives no return for his efforts. The probationary marriage and the return of the trial bride to her parents in no way affects her chances for a successful marriage, for she may soon be chosen by another hunter, and this time prove her desirability by bearing children.

In the region of Bering Strait and the Arctic Coast the girl's consent is necessary before a trial marriage may be arranged. The young hunter in all cases must have merited the title of hunter by delivering his first kill of each kind of animal to the oldest man or chief of the village, who divides it into as many parts as there are innies in the village. Each home group receives a portion while the young hunter receives some undesirable part of the animal or none at all. Once he has qualified in this manner, he is accepted by the hunters of the village as one of them, and is then in a position to marry. When arrangements for the trial marriage have been concluded with the prospective father-in-law the young hunter retires to the cosgy,[6] where he awaits acceptance or rejection by the girl he has chosen. If within due time she brings into the cosgy and sets before him a platter of choicest foods she thereby indicates her acceptance. Grasping the wrist of the young man she guides his hand to and from the platter while he eats with as much gusto as will indicate the degree of his pleasure. Thereafter the young hunter takes his trial wife to his isolated hunting camp or, as frequently happens, he goes to live with the girl in her parents' innie. If within the period of probationary marriage a child should be born to them the marriage is thereby consummated. If no children are born to them the husband is free to return the girl to her parents and select another. The length of the probationary period is controlled by the shortage or plentifulness of girls in the colony. If a shortage of females prevails a father may exact a greater price for his daughter, and if there are plenty of females there is less competition, which results in a much shorter trial marriage, and less service on the part of the young husband.

Courtship

It will be seen in the foregoing paragraphs that courtship and love-making take place during the probationary marriage period. The western Eskimo, although capable of affection, is undemonstrative. There can be no doubt that a successful marriage promulgates a sense of love and affection between mates, but there is no marked development of this relation prior to marriage. Mr. Hall bears out this statement in his assertion,[7] "Love—if it comes at all—comes after the marriage." The folktales of the western Eskimos bring to light very little regarding love matches, but do deal very frequently with marriage arrangements, freak marriages and pertinent ceremonies. In the case of the Eskimos of the Bering Strait region it is apparent that a sort of courtship exists when the chosen girl carries food to and feeds her promised husband in the cosgy. This is usually accompanied by a considerable demonstration on the part of the male, just as a peacock struts before his mate. Undoubtedly it is accompanied by, or more likely caused by, an impelling sex impulse which might be termed courtship. Throughout their legends and stories there comes to light the functioning of the instinct to show off before the opposite sex, and the same instinct functions in their lives to-day. This does not lead to open courtship or love-making, but it does have a definite bearing on the matter of the young

[6] *Cosgy*—The village meeting place built on the order of a very large innie.

[7] Hall, "Arctic Researches," p. 568.

hunter's winning the girl he wants. If two men should select the same girl at the same time there must follow a series of contests in which the winner takes the girl and the vanquished seeks another. Furthermore, courtship of a girl almost always includes the courtship of her parents' favor. To accomplish this, the young hunter endeavors to perform some outstanding act of valor, some singular success in hunting or some difficult feat of endurance. In their legend[8] "How Quakseetko Won a Wife," from the Bering Strait Eskimos, we discover a vivid portrayal of the type of courtship, which was no doubt the usual procedure. Among the Eskimo girls and boys of lesser prominence the process followed the same general form, but was much less romantic.

Influence of the Whites

In those parts of Alaska where the whites have contacted the Eskimos for a comparatively long period the blood of the Eskimos has been so thoroughly mixed with the blood of the white race that one would have great difficulty in identifying a full-blood Eskimo. By reason of their practise of sexual freedom the Eskimo girls are exceptionally gullible to the advances of unscrupulous white men. Furthermore, they have quickly recognized the marked improvement in the mental and physical characteristics of their offspring when they cross their own blood with that of the white man. Except in the primitive Eskimo villages this mixing of bloods has taken place to such an extent that the Eskimo girls now consider it an honor and something very desirable to bear a child by a white man, and it matters not whether in wedlock or out. I have in mind three specific cases in which the desire to bear children by white men became so impelling that the Eskimo girls deliberately requested cohabitation. In one of these cases the refusal of the white man highly incensed the girl and her parents as well. This attitude is not so prevalent among the mar-

A YOUNG ESKIMO WOMAN OF THE BERING STRAIT REGION. THE BERING STRAIT ESKIMOS ARE BOTH PHYSICALLY AND MENTALLY THE HIGHEST TYPE OF THE WESTERN ESKIMOS. WHEN OOGANEESEE TAKES A HUSBAND SHE WILL BE WED IN THE WHITE MAN'S CONVENTIONAL WAY, NOT IN THE TRADITIONAL WAY OF HER PEOPLE.

[8] "Folk Tales of the Western Eskimos," by Clark M. Garber.

ried women, yet their chastity may, in many cases, be purchased for a few trinkets. However, in my examination into the effects of deceased Eskimo women, I have found numerous trinkets, such as buttons from sailors' uniforms, beads, brightly colored glass trinkets, etc., which indicate a rather frequent adulterous cohabitation with white men. I have also been informed by many of the old men that the white sailors often took their wives from them by force and by first getting the men insensibly drunk. Although the crossing of bloods usually takes place between white men and Eskimo women, I know a number of cases in which white women have crossed their blood with the blood of Eskimo men.

Polygamy, Monogamy and Polyandry

Polygamous marriages are by no means uncommon among the western Eskimos, but as a general rule the Eskimo is monogamous. There are many cases in which a chief or medicine man becomes affluent to such a degree that to maintain his prestige and large household he must have a second or third wife. If a man's wife should prove incapable of bearing children, or if she should bear only female children, it is his privilege to take a second wife for child-bearing purposes, while the more menial tasks are relegated to the first wife. The practise of infanticide, with special reference to female children, regulates the number of women in the colony, and the practise of infanticide in turn is regulated by economic conditions, such as famine and starvation. During the lifetime of an Eskimo mother it is not uncommon for her to bear as many as ten or twelve children, of which number a possible four or five may reach maturity. In some of the remote colonies where primitive life still prevails, the infant mortality rate often reaches 75 per cent. Murdock found,[9] "As is the case with Eskimos, most of the men content themselves with one wife, though a few of the wealthy men have two each. I do not recollect over half a dozen men in the two villages who had more than one wife each." Throughout their legends and stories the western Eskimos make many references to the polygamous marriage. To them a man who could afford two wives was indeed a great man, and the man who had three wives was looked upon in a worshipful attitude, not because of the three wives, but because he must be the acme of power and wisdom.

In this connection allow me to cite a case of a most unusual nature, one which lies within the author's experience. Upon arriving at a primitive coastal village I was met by an Eskimo man who was apparently about fifty years of age. This man seemed very anxious to have private conversation with me, so as soon as my dog team was properly cared for he was given the opportunity to make his wants known. Being their atanak (big chief) they depended upon me for advice in all matters, even in their domestic affairs. This man wished advice upon the following proposition. It appears that he had married a girl who was about sixteen, whereas he was between forty-five and fifty. She bore one child for him, after which he apparently became sterile. His inability to produce more children weighed heavily upon him, because to die without leaving plenty of children would be a very bad thing. He therefore wished my approval on a plan which he had evolved to alleviate his childless situation. He proposed to induce a healthy young man of the village to live for a time at his home and to cohabit with his wife until she should be with child. Because I

[9] Murdock, Ninth Annual Report, Bureau of American Ethnology, p. 411.

realized what such an arrangement would mean in the way of domestic happiness I gave my approval. On two separate occasions did this take place, resulting in the birth of two fine boys to the young wife. Thereafter the young man accepted his fee from the proud husband, and before long had a wife and family of his own. The tradition that a man must not die childless is, without a doubt, largely responsible for the existence of the Eskimo race to-day. This same sort of problem had been solved by many others in the same manner, so I was informed by the man whom I had favored. Here again we find concrete support for the statement that all the Eskimos' traditions and customs which have to do with sex, marriage and childbirth function toward one purpose—the propagation and perpetuation of the race. In this case we have seen a domestic difficulty solved in a primitive, but most logical and economical way.

Polyandry is much less common among the western Eskimos than polygamy. I have known several cases of polyandry, but always the people were beyond the age of reproduction. This is accounted for by the fact that there is more often a shortage of females than of males. Some authors believe that Eskimo women live much longer than men, basing their belief on the fact that men are exposed to greater hardships and stronger devitalizing factors during their lifetime. On the other hand, we must take into account the fact that an equally large number of Eskimo women die early in life as a result of childbirth, post-partum hemorrhage and infection. Furthermore, Eskimo women show the symptoms of old age much earlier in life than the men, because they have suffered the ravages of many childbirths and the devitalizing effect of suckling their children until they are four and five years old. As far as the practise of polyandry is concerned, I am confident that such cases are exceptions rather than customary. If two men marry and live with one woman the cause is an economic one. Every Eskimo man must have a woman to sew his garments and make his mukluks. If there is a shortage of women, then one woman must serve two men in that capacity.

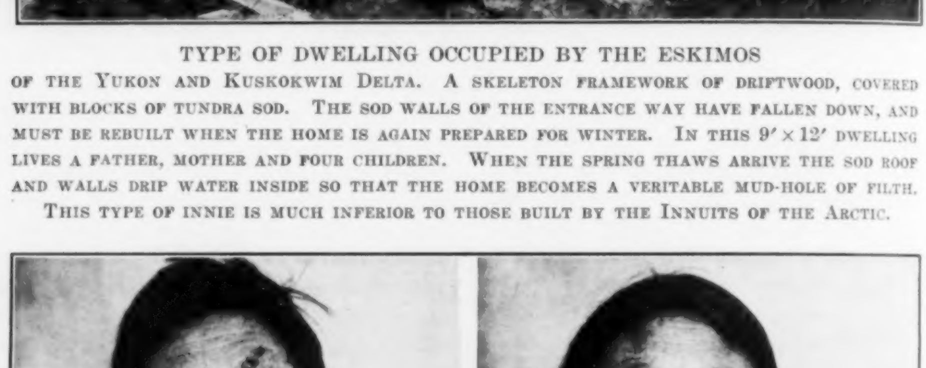
Divorce

As a rule marriage, when once consummated by childbirth, is a permanent relation. However, marriage ties are just as easily broken as they are made. No special ceremony is required in case a divorce is necessary or desired. If a man find his wife to be a nag and scold he needs but expel her from his innie and send her back to her parents. I have observed that most cases of divorce and separation occur between older men and women, *i.e.*, widowers who have married widows. A man who marries a woman who bears children for him will endure much nagging and scolding before he will expel the mother of his children from his innie. Also the mother of a family will stand much abuse before she will leave her husband and children. In their children they have a common interest, which results in greater tolerance and in most cases develops into a real affection between man and wife, and in both of them for their children. Space does not permit a disclosure of the innumerable ramifications of the Eskimos' domestic relations. It will suffice to say that children, especially boys, represent the criterion of a happy home. The absence of children breeds discontent and ultimately divorce, or the marriage of a second wife for child-bearing purposes. Regarding divorce, Murdock says,[10] "Easy and uncere-

[10] Murdock, Ninth Annual Report, American Bureau of Ethnology, p. 412.

TYPE OF DWELLING OCCUPIED BY THE ESKIMOS

OF THE YUKON AND KUSKOKWIM DELTA. A SKELETON FRAMEWORK OF DRIFTWOOD, COVERED WITH BLOCKS OF TUNDRA SOD. THE SOD WALLS OF THE ENTRANCE WAY HAVE FALLEN DOWN, AND MUST BE REBUILT WHEN THE HOME IS AGAIN PREPARED FOR WINTER. IN THIS 9′ × 12′ DWELLING LIVES A FATHER, MOTHER AND FOUR CHILDREN. WHEN THE SPRING THAWS ARRIVE THE SOD ROOF AND WALLS DRIP WATER INSIDE SO THAT THE HOME BECOMES A VERITABLE MUD-HOLE OF FILTH. THIS TYPE OF INNIE IS MUCH INFERIOR TO THOSE BUILT BY THE INNUITS OF THE ARCTIC.



YUTE MAN AND WIFE FROM THE KUSKOKWIM RIVER

THE SPARSE BEARD AND PRONOUNCED MUSTACHE OF THE KUSKOKWIM RIVER ESKIMO ARE HIS MOST DISTINGUISHING CHARACTERISTICS. THEIR COMPLEXION IS SOMEWHAT LIGHTER THAN THOSE OF THE ARCTIC, AND THEIR STATURE A LITTLE SHORTER. APPROXIMATE AGES 50 YEARS EACH.

monious divorce appears to be the usual custom among the Eskimo generally, and the divorced parties are always free to marry again." The folk tales of the western Eskimos are veritably full of references to divorce, separation, marriage, sex and childbirth. To them such things were no more improper than the simple process of wicking the seal-oil lamp. They were common, everyday occurrences in their lives, and they looked upon them in no other sense. Joking and badinage about sex and domestic life had no more significance than joking about the failure of a hunter.

Inbreeding

Until the ingression of the white man the Eskimo did not realize the benefit of blood-crossing. It was, and is to-day in the primitive villages, traditional that marriages should be confined to the immediate locality. In their folk-tales they frequently refer to the mating of young men and women with strange people in faraway places. The characteristic legendary outcome of such marriages is unhappiness and often tragedy. In view of the fact that their traditions and folk-tales go hand in hand it is not difficult to deduce that marriages with strangers from other and distant villages were frowned upon. In an effort to determine the extent of inbreeding the writer attempted to trace the genealogical connections of a prominent Eskimo family on the Kuskokwim River. The result showed that every other Eskimo family in the colony and vicinity was closely related to this family. It is common for cousin to marry cousin. I have known cases where uncles married nieces, and aunts married nephews. And it is not extremely rare that brother married sister. In four cases of which I have record a father married his daughter. Such close intermarriages are frequently mentioned in their legends and stories, but always with a large degree of distaste and condemnation. No penalty is attached to the marriage of a father with his daughter, etc., but it is strongly disapproved and unfavored by the people.

Celibacy

Celibacy, as a virtue, has never been adopted by the Eskimo people. Unless there is a marked shortage of women in the Eskimo colony not even the poorest man need be without a wife. Such shortages of females have occurred in times past and once during the author's life among the western Eskimos. The chief cause is the destruction of female babies as an economic measure during times of famine and starvation, so that not enough of them reach maturity. To overcome this difficulty a polyandrous arrangement was developed by which one woman would serve two men. In the 120 Eskimo colonies visited by the author not a single instance of celibacy was discovered. It may therefore be safely concluded that celibacy was unknown among the western Eskimos.

Exchanging and Loaning Wives

The exchange and loaning of their wives is a custom which is practised not only by the western Eskimos, but it is found among many of the earth's primitive peoples. It may take place as an amusement, as a matter of deference and hospitality shown to distinguished visitors, or the reason may be a practical one. Among the western Eskimos all three of these factors are responsible for the practise. In the region of the Yukon and Kuskokwim Delta the author frequently found himself honored by a village chief offering his wife as a bedfellow during the duration of the visit. This was his customary way of showing his respect and hospitality to visitors prominent among his people. Refusal of this offer is a base insult, which can

be assuaged only by presenting the host with a very fine gift. Murdock[11] speaks in the following manner concerning this practise among the Point Barrow Eskimos, "A curious custom, not peculiar to these people, is the habit of exchanging wives temporarily. For instance, one man of our acquaintance planned to go to the rivers deer-hunting in the summer of 1882, and borrowed his cousin's wife for the expedition, as she was a good shot and a good hand at deer-hunting, while his own wife went with his cousin on a trading expedition to the eastward. On their return the wives went back to their respective husbands. The couples sometimes find themselves better pleased with their new mates than with the former association, in which case the exchange is made permanent."

Among the Eskimos of Bering Strait, when the chief of one village visited the chief of another village, his host always loaned him a wife for the duration of his visit. If more than one person came to visit at the same time a consort was provided for each of them. This custom is often mentioned in their folk-tales. For instance, in the Legend of the Magic Pooksak we find a direct reference to this custom.[12] "The three wives of the famous chief Pumimyouk had been given to the visitors to be their partners or mistresses during their visit at Engyukarik. When the feast was over the three women departed each taking the man she had fed."

Jealousy

Jealousy among the western Eskimos, as among all people, is based upon the desire for exclusive possession of the mate. Westermarck[13] says, "Among the Eskimos the jealousy of the men seems to be feebler than among most other natives of America; but it is not absent." I have found during my life with the primitive and semi-civilized western Eskimos that the jealousy of the men and also the women is rather pronounced. After a wife is taken and children are being produced, the husband jealously guards the chastity of his mate. Jealousy in the Eskimo husband begins as soon as he has taken a wife in probationary marriage, and when proof of his wife's fecundity has been attained he demonstrates a deep concern for her chastity. While the young men and women of the village exercise sexual freedom prior to marriage there is little evidence of jealousy among them other than that of a petty nature. In a number of cases which I have observed the best girls of the village who would make the most likely wives were freely satisfying the sensuous desires of the young men, yet there was no evidence of jealousy among them. But as soon as one of these girls was taken to wife her husband constantly guarded her against the approaches of any of her former paramours. Even in polygamous marriages the same concern is shown regarding the chastity of all wives. This accounts in a great measure for the custom of the young husband leaving his wife with her parents or taking her with him to an isolated camp. Eskimo wives are equally jealous of their husbands, especially if the husband is a famous hunter and good provider.

Let me here cite an instance which will illustrate clearly the Eskimo husband's jealousy. In a village on Bering Strait there lived a man who was the husband of a fine type of Eskimo woman and the father of six equally fine children. There also lived in this village a man who, because of his previous relations with white people, had acquired a considerable degree of sexual licentious-

11 Murdock, Ninth Annual Report, Bureau of American Ethnology, p. 413.

12 "Folk Tales of the Western Eskimos," Clark M. Garber.

13 Westermarck, "The History of Human Marriage," Vol. I, p. 307.

ness. By reason of his superior intelligence most of the men feared him and would tolerate his tampering with their women, regardless of their jealous hate of him. One day upon returning from his trap lines, the father of this fine family learned that this sexual pervert had cohabited with his wife. The violation of his wife right before his own family so stimulated and wrought upon his animal instinct of mate protection that he came to me in a great frenzy of jealous hate and demanded that I give him permission to eliminate the offender. Upon my refusal, the wronged husband informed me that if this man visited his wife again he would kill him, whether I gave my consent or not. Murders have been committed and feuds thereby started as a result of jealous rage on the part of Eskimo husbands whose wives have been incontinent.

In concluding this discussion the author wishes to stress the point that the acquisition of accurate ethnological data concerning the western Eskimo depends on the actual observation of the manifestations of their life process. In our day it is rarely possible to find a native people whose life is untouched by European influences. To acquire ethnological information of value concerning such natives the ethnologist must have the ability to differentiate between the material resulting from European influence and the purely native culture. He must be prepared to go and live for a time among the natives whom he would study. He must go among those natives where the European influence has not been felt, or where it has been felt the least. And it is no less important that he go with a clear understanding that to emphasize race difference will cause the natives to despise him. Only by adapting himself to the simple life of the native people, by gaining their utmost confidence, will he be successful.

EVOLUTION OF THE USE OF THE MODERN MATHEMATICAL CONCEPT OF GROUP

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WIDELY different meanings are ascribed to the mathematical concept of group in our current dictionaries and encyclopedias.[1] Some of these are so general that a large number of apparently fruitless systems of abstract elements come thereunder. For instance, the three elements represented by 1, 2, 3 have been called a group[2] when their combinations are determined by the following elementary table:

$1 \cdot 1 = 1$	$2 \cdot 1 = 3$	$3 \cdot 1 = 2$
$1 \cdot 2 = 3$	$2 \cdot 2 = 2$	$3 \cdot 2 = 1$
$1 \cdot 3 = 2$	$2 \cdot 3 = 1$	$3 \cdot 3 = 3$

Hence these elements obey the commutative but not the associative law when they are combined by multiplication, and the table may therefore be of interest even to the student of elementary algebra as an illustration of the nature of the associative law in multiplication. It is, however, questionable whether the term group should be available for speculations which are so foreign to what is commonly regarded as useful mathematics as contrasted with mathematical recreations.

Those who are especially interested in group theory would naturally like to maintain the respect for this subject which it has secured by its wide usefulness. To do this it seems desirable to insist on a definition which excludes matters of little or no importance and which is also sufficiently restrictive to give rise to extensive developments. Such a definition was given more than forty years ago and can be expressed in the following brief form: A given set of distinct elements is a group if they obey the associative law when at least three of them are combined, two at a time, and which are such that if two of the symbols in the equation $xy = z$ are replaced either by one or by two elements of this set, the resulting equation has one and only one root in the set. In what follows we shall assume for the sake of definiteness that the term "modern mathematical concept of group" implies either this definition or one which is equivalent thereto, unless the contrary is explicitly stated.

This definition is obviously somewhat more complex than the supplemental one which was at first used in connection with the theory of substitutions, *viz.*, that a set of distinct substitutions is a group if it includes the square of every one of them and also the product of every two. It should, however, be observed that the three elements noted in the first paragraph satisfy this definition of a substitution group. What is more important is the fact that the definition noted at the close of the preceding paragraph has led to developments of such extensive usefulness with respect to abstract elements that it seems to have justified itself by experience. While the condition that a set of elements contains the square of every one and the product of every

[1] In Webster's New International Dictionary, including the second edition (1934), there appears under the entry "distribution" the following statement which is misleading from the standpoint of mathematics: "Arrangement of objects in classes called *parcels* when the order is indifferent, and *groups* when it is essential." The direct definition which appears under the entry of "group" in this dictionary is inadequate as regards the theory of abstract groups but is useful in the study of certain sets of concrete elements, where it becomes a supplemental definition.

[2] J. Perott, *American Journal of Mathematics*, Vol. 11 (1889), p. 101.

two has also proved to be very fruitful when applied to certain concrete elements it has little value with respect to an abstract theory as is illustrated by the cited example.

One of the most striking facts in the history of group theory is that in the study of concrete elements some writers imposed certain fructifying conditions and spoke later of these added conditions as if they were the characteristic properties of a group. This procedure was followed in the beginning of the subject as regards the theory of substitutions and was employed later even as regards less restrictive sets of elements. Since it has been the source of much progress it should not be condemned, even if it has also been the source of much confusion. It seems, however, reasonable to strive to secure progress without confusion in this as well as in other fields of scientific endeavor, notwithstanding the fact that progress with confusion is more desirable than no progress. At any rate, it is of some interest to trace the early use of a concept in its restricted form even if the nature of these restrictions were not emphasized at the time and were not formulated until thousands of years had elapsed since the concept was first used.

The use of the group concept has a much longer history than its definition and hence the present article relates to much earlier developments than the one which appeared in a previous number of this periodical[3] under the title, "Evolution of a Modern Definition of the Mathematical Term Group." In an implicit form the modern concept of group underlies the prehistoric divisions of the day into equal parts, such as its division into 24 hours, where 24 is a kind of modulus in the modern sense of the word and constitutes a type of zero with respect to addition. The use of this type of zero is much older than the use of a special symbol for zero as a number and *the addition group with respect to an implied modulus is one of the earliest instances of the use of the modern mathematical concept of finite group,* which emerged slowly from an implicit to an explicit concept.

A somewhat trivial example of the prehistoric use of this concept is furnished by the operation of subtracting from unity. It is well known that complementary fractions, that is, fractions of the form $1/n$ and $(n-1)/n$, play a prominent rôle both in the language and in calculations of the ancients. The two fractions $\frac{1}{3}$ and $\frac{2}{3}$ are especially noteworthy along this line, while $\frac{1}{2}$ is its own complement. Special symbols appear in the ancient Egyptian as well as in the ancient Babylonian literature for a few of these complementary fractions, and they seem to have been regarded very early as being on a par with the system of unit fractions. In this connection it is to be emphasized that two complementary fractions constitute a group of order 2 with respect to the operation of subtracting from unity. The operation of dividing unity by a given number later gave rise also to the group of order 2 and when these operations were combined still later they gave rise to the important group known as the anharmonic ratio group.

The earliest examples of the concept of group of infinite order in the modern sense are also prehistoric. One of these is furnished by the totality of the positive rational numbers when they are combined by multiplication. The development of these numbers has been regarded as the main mathematical objective of the two leading pre-Grecian mathematical civilizations, *viz.,* those of the ancient Babylonians and the ancient Egyptians, but it was only partially attained by them, although the former made much more progress in this direction than the latter as a result of their development of an approximately positional system of arithmetic having 60

[3] Sci. Mo., 39: 554–559, 1934.

for its base.[4] One of the striking features of this system is that it has no symbol which separates the integers and the proper fractions when they are represented systematically, corresponding to our modern decimal fractions.

While some groups in the modern sense of this term were used in prehistoric times the claim that there existed a prehistoric group theory, which is emphasized in the introduction to the second edition (1927) of A. Speiser's widely used "Theorie der Gruppen von endlicher Ordnung," can not be said to be as yet fully established even if many of the early ornaments seem to point to a domination of the group concept in view of their types of symmetries. There are many evidences pointing to the influence of the concept of symmetry on the early mathematical developments. In particular, several of the quadratic equations in one unknown considered by the ancient Babylonians resulted from the combination of two symmetric equations in two unknowns in the forms of $x+y=a$ and $xy=b$, respectively, and the first proposition of Euclid's "Elements" relates to the symmetric equilateral triangle. There is, however, a long step from an emphasis on symmetry to the development of a group theory in the modern sense of this term.

Special subgroups of the group of movements of space were also employed implicitly in prehistoric times. For instance, the group of rotations around a fixed point was naturally suggested by the daily apparent movements of the sun and the seasonal changes of the weather. In a world which is so replete with groups as ours is the development of scientific thinking naturally led to a consideration of certain groups, but these were not known to be sufficiently abundant to merit a special theory before the latter part of the eighteenth century, and then only because they furnish a means of classifying functions which are useful in the study of the algebraic equations in one unknown. It is then that a stone which had been previously overlooked by the builders of mathematics was made a cornerstone which has since then been used with increasing appreciation.

The main object of the present article is to direct attention to the *evolution* of the use of the modern mathematical concept of group. In the ancient groups to which we have referred this evolution was usually completed in prehistoric times, and these groups are of less interest in the present connection than those whose evolution was completed in historic times. One such group is the modern addition group embodying the use of both zero and negative numbers. This group was not completely developed until after the beginning of the Christian era. In fact, it was not used even by the ancient Arabian mathematicians although the Hindus had employed it earlier with some success. It was naturally used with some hesitation before the use of the negative numbers in multiplication was explained satisfactorily about the beginning of the nineteenth century even if its theory did not directly depend upon this explanation.

Another group whose evolution was completed in historic times is the one now commonly known as the group of movements, which is intimately connected with the foundations of elementary geometry. As positive and negative movements are considered therein it is very closely related to the addition group to which we referred in the preceding paragraph. This group is especially interesting in the history of mathematics because the study of all its subgroups by C. Jordan (1838–1922) constitutes the earliest extensive direct use of groups of infinite order (1867). Before

[4] O. Neugebauer, "Vorlesungen über Geschichte der Antiken Mathematischen Wissenschaften," 1: 203, 1934.

the zero movement and negative movements were considered various groups of movements with respect to a modulus, and semigroups of movements corresponding to semigroups of addition, engaged the attention, at least implicitly, of those who developed mathematical subjects. The later evolution relates largely to the emphasis on the identity (zero movement) and the inverses (negative movements) in the group of movements.

An elementary group whose evolution belongs to a comparatively recent period is the group of multiplication, modulo m, whose elements are the natural numbers which are not greater than a given natural number m and prime to m. This has been called the most important example of abelian groups of finite order[5] and was first employed effectively by L. Euler (1707–1783). It should be observed that this abelian group was employed (1760–1) before J. L. Lagrange (1736–1813) studied substitution groups in connection with his theory of the solution of algebraic equations, but the latter work influenced the early development of group theory more than the former, as a result of the fact that the theory of substitution groups received attention before that of abstract groups. J. L. Lagrange used the concept of holomorph of a cyclic group.

Not only were various groups in the modern sense of this term used by the ancient and the medieval mathematicians, but these also began early to observe evidences of the (1, 1) correspondence between two of the most important ones of them, *viz.*, those formed by a general arithmetic progression and by a general geometric progression, respectively. This made it possible to coordinate the fundamental operations of addition and multiplication and gave rise later to the use of logarithms during the seventeenth century, a subject which has ever since then been highly regarded, especially in those applications which involve lengthy numerical calculations. It is an interesting fact that J. Napier (1550–1617) did not make the identities of these two groups correspond when he constructed his so-called tables of logarithms by means of a certain (1, 1) correspondence between these two groups.

Hence his tables were soon replaced by others in which the identities of these two groups correspond, and on this account the latter are more convenient. It is a striking fact that this important feature of the history of logarithms has been commonly overlooked by the writers of general histories of mathematics up to very recent times. The first such history in which it is explicitly noted seems to be that of J. Tropfke's "Geschichte der Elementar-Mathematik" and here it did not appear before the third edition of volume 2 (1933), page 206. The history of the logarithms becomes much clearer when this point is emphasized and the subject of group theory thus exhibits its usefulness as a suggestive and coordinating theory extending down to the most elementary parts of mathematics. Implicitly it was a dominating concept long before the nature of this domination was explicitly noted.

The (1, 1) correspondence between the groups formed by the general arithmetic progression and the general geometric progression began to be noted by the ancient Greeks, in particular, by Euclid and Archimedes, as regards the product of numbers expressed as powers of the same number. This seems to furnish the oldest example of a (1, 1) correspondence between two infinite groups and is important because the (1, 1) correspondences between groups constitute now a fundamental problem in their theory. These correspondences are now commonly known as simple isomorph-

[5] H. Weber, "Lehrbuch der Algebra," vol. 2 (1899), p. 60.

isms, and they were not studied as a special subject before the second half of the nineteenth century. The idea of simple isomorphisms underlies the development of abstract numbers and hence it is one of the primitive notions in the development of mathematics. The fact that it did not receive a special name before the explicit use of group theory is another evidence of the fact that definitions of concepts have frequently lagged far behind their use in the development of mathematics.

There are three stages in the use of the modern mathematical concept of group which should be noted. The first of these relates to the period when this concept was used without any definition of the term group. This period extends into the first half of the nineteenth century. It was followed by a period of much greater mathematical activity during which the concept was used as a supplemental concept with respect to concrete elements. This period extends up to the latter part of the nineteenth century, when various writers began to use the concept as an independent concept, while others continued to use it as a supplemental concept. The writings of E. Galois (1811–1832), A. L. Cauchy (1789–1857), C. Jordan (1838–1922), S. Lie (1842–1899), F. Klein (1849–1925) and H. Poincaré (1854–1912) illustrate this use of the group concept, and they contributed most forcibly towards popularizing this concept by exhibiting its wide usefulness in establishing contacts which had theretofore escaped explicit notice. These writings illustrate a striking feature of the devolpment of the mathematical language, *viz.*, the use of terms which are only partially defined and which gain in content with the advances in the fields to which they relate. The term mathematics itself is another example of such a word.

The third stage in the use of the modern mathematical concept of group is represented by systems of postulates which make it possible to use the concept with respect to abstract elements and thus to construct an abstract theory of groups. The rapid growth of this theory is due to the fact that many of its fundamental theorems had been developed under the influence of the supplemental definition of the term group and required only a slight change of language to be available for the abstract theory. Just as in other parts of mathematical development, so here unexpected richness of results came into mathematics not so much because their sources were diligently sought by the investigators but because they unexpectedly forced themselves on their attention. In the words of F. Klein,

> One cannot repress that oft recurring thought that things sometimes seem to be more sensible than human beings. Think of it; one of the greatest advances in mathematics, the introduction of negative numbers and of the operations with them, was not created by the conscious logical reflection of an individual. On the contrary, its slow organic growth developed as a result of intensive occupation with things, so that it almost seems as though men had learned from the letters.[6]

Groups are among the things which sometimes seem to be more sensible than human beings, for they have frequently suggested properties which had not been suspected both as regards their own structure and as regards the concrete objects to which they were applied. The inherent suggestiveness of mathematics is clearly illustrated also by the extension of the number concept through the study of all the possible roots of algebraic equations. While this inherent suggestiveness relates to a variety of mathematical subjects it is especially pronounced in group theory so that H. Poincaré was led to say near the close of his brilliant career, "The theory of

[6] Felix Klein, "Elementary Mathematics from an Advanced Standpoint," Vol. 1, p. 27, 1932.

groups is so to say the whole of mathematics divested of its matter and reduced to pure form." Such outbursts of enthusiasm are unusually common in the literature of group theory and exhibit its attractiveness when its various applications are fully comprehended.

Since the concept of groups is now in the final stage of its metamorphosis towards the abstract and has in recent times passed rapidly from the stage of an undefined concept to one which was at first used only with a supplemental definition, it presents at present an interesting subject also from the standpoint of mathematical history. These changes help to explain the widely different definitions of this concept adopted by recent writers and make it somewhat difficult for those who are not specially interested in this field to decide which of these definitions they should use. It should, however, be emphasized that it seems more desirable to adopt one which is in line with the abstract towards which the trend is moving than one which is based on the supplemental idea from which the trend is moving away. The emphasis here is, however, on the fact that we have before our eyes in contemplating this concept an interesting example of the metamorphosis towards the abstract of a mathematical concept, which may be of interest to the student of the devolpment of scientific thinking.

It may be of interest to refer in closing to a recent extension of the modern mathematical concept of group. If a set of h distinct group elements, s_1, s_2, . . . , s_h, has the property that it contains the product of every two of them, including the case when these two elements are equal to each other, then this set is called a group of order h. An obvious extension of this concept is that the set includes the product of every $n+1$ of these elements, including repetitions, but does not include the product of a smaller number of them. In the special case when $n=1$ this reduces to the one considered above but for all larger values of n it follows that the set is not a group but a co-set of a group of order nh with respect to an invariant subgroup of order h. While the term co-set was introduced to represent a supplemental concept relating to a group it results from what precedes that the concept of group may also be ragarded as supplemental to that of co-set. Such a co-set becomes really fruitful only when it is considered in connection with the related group and hence the latter dominates here also.

ISOSTASY

By Dr. WILLIAM BOWIE

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We usually confine our engineering thoughts to such structures as bridges, buildings, dams, tunnels, highways, power projects, etc., but I like to extend engineering thought to the earth as a whole. We really do not give very much consideration to the old earth and what has been going on in its interior and at its surface during the past billion or so years. I speak of billions of years. I think I am justified in this, for according to all the geological and geophysical evidence the earth's surface began to be disturbed about a billion six hundred million years ago. It was then that erosion and sedimentation began. We haven't the slightest indication of what was going on prior to that time, but it is reasonably certain that at the beginning of what we may call the sedimentary age the earth's surface was irregular. There were great areas that were depressed and others that were elevated. The hollows or low places were filled with water forming oceans and seas, and the high land projected above the water to form continents and islands.

What caused the irregular surface of the earth originally? No one knows. There have been many hypotheses advanced, but most of them are very unconvincing. The best one of these hypotheses, according to my view, is that which calls for the formation of the moon by a disruption of a portion of the earth. If an outer shell of the earth had solidified and its period of rotation had become such as to synchronize with the natural elastic period of the earth as a whole, then perhaps the mass would have been unstable and a drop would have been thrown off, that drop being the moon.

It is rather interesting to note that the density of the moon, 3.4, is just about the density of crustal rock on the earth. The volume of the moon is about equal to the volume of crustal material, over the areas of the oceans, approximately one hundred forty million square miles, to a depth of thirty or forty miles. It is possible that the moon may have been thrown away from the earth and that the great scars which were left are now occupied by the oceans.

In any event, we know that a billion and a half years ago the earth's surface was irregular. If it had not been irregular, the water now forming the oceans would have been at a uniform level over the whole surface and, therefore, there could not have been any such thing as erosion and sedimentation.

The Coast and Geodetic Survey has taken an active part in testing a very interesting idea to the effect that the earth is composed of materials of two characteristics. The first is a great inner body which is apparently solid but with no residual rigidity; that is, it will change its form without breaking under stresses that act for long periods of time. The second material composes the crust or outer part of the earth. It has been found by the analyses of geodetic data that this crust extends to a depth of approximately sixty miles below sea level. The crustal rock has residual rigidity. It breaks if the stresses exerted on it are too great.

The crust is like a huge blanket sixty miles in thickness and 197 million miles in area wrapped around the earth. The geodetic data show that this blanket is resting on the interior material very much as an ice sheet is resting on a lake or on the Arctic Ocean.

There have been enormous quantities

of material eroded from continents and carried down to tidal waters. An estimate by the scientists of the U. S. Geological Survey is to the effect that a blanket of material one foot in thickness is carried from the United States to tidal waters each nine thousand years. This means that at that rate a mile of erosion would occur in approximately forty-five millions of years. Since the sedimentary age is thirty times that long, there may have been thirty miles of material eroded from the United States, if the area was maintained at its present elevation.

It is, of course, unlikely that any such amount of erosion could have taken place from any one spot. As erosion occurs in a mountain area the crustal blanket is lightened at this point. It doesn't press down on the nucleus quite so heavily and, therefore, the nucleus, yielding to these stresses, will well up under the area of erosion and push the crustal material upward. Where the sediments have been deposited the crustal blanket becomes extra heavy and it pushes down on the subcrustal material, which yields and moves to the side, just exactly as water would be displaced if an ice cake or a portion of an ice field were weighted down by an added load.

Sediments have been deposited during so-called geological time to thicknesses of as much as 40,000 feet, nearly eight miles. One can imagine what a great load that is and how the crust beneath would break or yield under it. There is evidence that in the past mountain areas have been completely leveled off. This has required much more material to be carried from the mountain area than the amount above sea level to begin with. If the density of the subcrustal material that enters crustal space is 10 per cent. greater than that of the surface rock, then it can easily be shown that ten times as much rock as that contained in the mountain must have been eroded to bring it to sea level.

It is also of interest to note that every plateau and every mountain mass has been formed in areas that have been previously subjected to great amounts of sedimentation. There must be some close relationship between uplift of portions of the earth's surface and sedimentation.

There is hardly a spot on the earth's surface that has not been below sea level at least once. This is indicated by the presence of fossils of marine animals in the stratified rocks. This process has not been finished. It must be going on to-day and it will continue to go on as long as we have sunshine and rain. The heat from the sun evaporates water which falls to the surface of the earth as rain. After every rain some material is carried from high to low areas as sediments or as matter in solution. While the process of wearing down a mountain or plateau is slow, yet there is plenty of time. A billion and a half years is quite a long period.

The earth should not be considered as a rigid structure. It is a yielding one. As loads are shifted at its surface the crustal blanket will go up or down according to whether it is eroded or overloaded. It is remarkable how nearly the crustal blanket is in equilibrium in all its parts. *Isostasy* is a term meaning equal pressure and is used to express the equal pressure of the earth's crust on the inner material. Of course, we do not expect each small portion of the earth's crust, say a mile or ten miles square in cross section, to be in perfect isostatic equilibrium, but if we could cut up the earth's crust by vertical planes into blocks approximately one hundred miles square, and if there were no resistance, frictionally or otherwise, to the blocks moving up or down, we should expect the movement of any one block to be very small, measured perhaps in hundreds of feet, certainly not in thousands.

The basis of this opinion is, of course, the vast quantity of geodetic data that

has been accumulated in this and other countries. We determine the longitude and latitude of thousands of points over the earth's surface by means of triangulation, and we observe the astronomical latitudes and longitudes at some of those places. When we compare the astronomical and so-called geodetic latitudes and longitudes, we find that in many cases they differ greatly. Some of them differ as much as thirty seconds of arc, a difference corresponding to as much as a half mile on the earth's surface. We know that the irregular surface of the earth has an effect on astronomical observations. All the astronomical work is referred to the direction of the plumb line, which is at right angles to the levels of the astronomical instruments. At different points on the same degree of latitude the plumb line does not point to the same spot on the axis of the earth. This is because the direction of gravity is influenced by the irregular terrain. A great mountain mass will deflect the plumb line toward it.

We have some notable examples of deflections of the vertical. One in our own area is on the Island of Puerto Rico. Two astronomical stations, one on the south and the other on the north coast of that island, were joined by triangulation. It was found that the exact distance determined by triangulation was just about a mile shorter than the distance computed from the difference in latitudes of the two astronomical stations.

We can correct the astronomical observations for the effect of the irregular surface of the earth. The masses above sea level will attract the plumb line, while the deficiencies of mass in nearby oceans will, in effect, repel the plumb line. After we make the corrections for the irregular surface of the earth, however, we find that the astronomical and geodetic latitudes and longitudes still stand apart. The corrected values differ in the opposite sign from what they did originally. It was this discrepancy that led to the thought that there must be a deficiency of density under mountains and plateaus and an excess of density under oceans. These compensating masses were assumed to be exactly equal to the masses of the topography, either positive or negative. A quantitative test of this theory was made by engineers of the Coast and Geodetic Survey some thirty or more years ago. When the effect on the plumb line of the compensating deficiency or excess was applied to astronomical observations, the astronomical and geodetic longitudes and latitudes were found to be very nearly the same. There were still some outstanding differences, but it has since been found that those differences are probably largely due to buried structure such as masses of igneous rock fairly close to the stations, or to great troughs filled with material of less than normal density that in effect repel the plumb line.

In addition to the astronomical stations we have values of gravity determined at many places over our own and other countries. We find that the gravity values, representing the pull of the earth, are influenced by the irregular terrain just exactly as the astronomical determinations of longitudes and latitudes are affected. We bring the observed and theoretical values of gravity more closely into accord when we apply corrections for topography and for the isostatic compensation of the topography.

It seems reasonable to assume that a mountain mass such as the Rockies is compensated within 10 per cent. I am of the opinion that the compensation is even closer than that for areas a few hundreds of miles square.

The isostatic condition of the earth's crust has been taken into account by engineers and mathematicians of the

Coast and Geodetic Survey in deriving the dimensions for the mathematical surface that most nearly fits the sea level or geoid surface of the earth. If water covered the earth's surface everywhere to the same depth and there were no differences in the distribution of densities along the radii of the earth and if this water surface were undisturbed by tides and winds, the surface would be very nearly a true spheroid. Any meridional section would be an ellipsoid and any small circle parallel to the Equator would be a circle. Owing to the irregular surface of the earth, however, these curves are not true mathematical ones. They are somewhat irregular. It is not known definitely just how much these irregularities amount to, but probably they are not greater than several hundred feet at any place on the earth's surface. It is reasonable to assume that the geoid or sea level surface is below the spheroid over ocean areas and above the spheroid over the continents. In deriving the shape and size (figure) of the earth from triangulation and astronomical data, we get very much better values, that is, values nearer the truth when we apply the principle of isostasy.

It is interesting to note that forces do not pile up to tremendous proportions in the earth's crust. The crust yields frequently. We do not know how many earthquakes occur each year, but the number that is recorded by the few seismological stations in existence is something like ten thousand. This is about three hundred earthquakes per day. Of course, many of these are so exceedingly small that they could only be detected from the records of the seismological stations, but they show constant yielding. It is fortunate that there are many earthquakes, for if it were otherwise, stresses would be set up in the earth's crust that would be so tremendous in size that when a break eventually came the destruction would be far greater than it is now.

Earthquakes have caused great damage to the works of man and have caused tens of thousands of deaths, but this has been due largely to lack of preparation for them. A wooden house or a modern office building with steel framework is not seriously damaged by an earthquake, but other types of structures, if put up without braces and designed only to withstand the weight of the material and the forces of the wind, may be rocked to pieces when an earthquake occurs. It is not costly to put in diagonal bracing to keep the walls and floors of a building strongly connected during vibration of the structure by an earthquake. This matter of protecting buildings and engineering structures from the effects of earthquakes is receiving very serious consideration by engineers to-day, especially by those operating in regions where earthquakes are most apt to occur.

There are several causes of earthquakes of which we are quite sure. One is the pressing down of crustal material in areas that are undergoing very rapid and great sedimentation. As the load is piled on the crust it will at first yield elastically. No doubt there is some plastic movement of crustal material involved, but at times the weight becomes greater than the elastic limit of the rock and then a sudden break or snap occurs. An earthquake is really a train of waves that go out from the place where the break occurs.

A second cause of earthquakes must be the moving up of crustal material under an area subject to erosion. As the material is eroded from a portion of the crustal blanket it becomes lighter. The subcrustal material moves upward into crustal space to balance the underloading. Sometimes the movement upward is so great that the rock snaps instead of becoming distorted to relieve the stress. When this breaking occurs, we have an earthquake.

There is a third cause of earthquakes. An area that has been forced down

under sediments will eventually rise up to form a plateau or a mountain. In this process of going up, the elastic limit of the rock may be exceeded and the resulting break will cause an earthquake. We also have the case of a mountain or plateau area where great erosion has taken place. Eventually the erosion areas will sink below sea level and form geosynclines or troughs. This sinking sometimes causes rupture of the rock.

There are many phases of geology that are extremely interesting, such as volcanoes, or outflows of lava, the formation of dykes, the over-thrusting and under-thrusting of rocks. Rocks are distorted in fantastic ways. It seems to me that engineers must work with geologists in order to discover just what is going on to change the configuration of the earth's surface and to cause all these very interesting phenomena. We should treat the earth as an engineering structure just as we would things that engineers build. We should search for causes of failure or yielding just as we would if a foundation started to give way.

I am confident that the cause of mountain and plateau formation is the change of density of the material of the crust below areas of sedimentation. Every cubic yard of crustal material under the sediments is pushed down into hotter regions, perhaps six or eight miles at times. Eventually the surfaces of equal temperature which have been depressed will rise to their normal positions. When they do the crustal material will become hotter and no doubt will take on new form or forms. There are many materials that appear in different forms and with different densities. Carbon occurs as a diamond, as coal and as graphite, with a very wide range of density. Quartz appears in many varieties with exactly the same chemical elements but with densities that vary as much as 6 or 8 per cent. Some of the feldspars having exactly the same chemical elements appear in different forms with quite large differences in density.

Why is it not possible for a change to occur in the density of some of the elements of the crustal material as this material is subjected to different temperature and pressure conditions? The old explanations for the formation of mountains and continents leave one rather confused, while this idea of change in density as the crustal material moves up and down seems simple and reasonable.

The question is frequently asked, what effect has all this movement of the earth's surface on surveying and mapping? The answer is that in some places it disturbs conditions very materially. In California, for instance, we find that some of our triangulation stations have been changed in their longitude and latitude by as much as ten feet, and along some of our lines of leveling bench marks have been changed by large fractions of a foot. Of course, there may be very great changes when an earthquake occurs. At Yakutat Bay, Alaska, the earthquake of 1901 made an extreme change in elevation of 49 feet. One can imagine what that would do to a river, highway or railroad.

We are now covering the country with arcs of triangulation and lines of levels, with the arcs and lines fairly closely spaced. Whenever an earthquake occurs we can go to the region where the break took place and rerun our lines of levels and reobserve the angles of the triangulation and determine just how much movement has taken place for each one of our stations. It will be interesting to learn how large an area is affected by an earthquake. Is it only a strip a mile or so wide or are hundreds or thousands of square miles affected? I am rather inclined to think that an earthquake is caused by forces of a very local nature, but I can not prove this. We shall have

to wait until we reobserve the triangulation and leveling that lie near the epicenters of many earthquakes.

I do not know just what happens to property lines when an earthquake occurs. In California, the earthquake of 1906 shifted fences in position by as much as ten or fifteen feet. Does the property line follow the fence or does the owner still have the same amount of area that he had before the 'quake? I believe that engineers of California have given some thought to the adjustment of property lines after earthquakes have moved them.

There seems to be very little evidence that the processes that operate on the earth's surface to change the configuration will end very soon. The earth's surface is certainly not getting very much colder than it was a billion years ago. Plant and animal life of those times could not have stood a temperature as great as that of the boiling point of water, 100 degrees Centigrade. I do not know what the average temperature of the earth's surface is now, but it can not be far from fifteen or twenty degrees Centigrade. We may assume that the earth's surface, if it has cooled down through the billion and a half years since rain began to fall and sediments formed, has not changed its temperature by as much as 100 degrees Centigrade. This is at a rate of one degree in approximately fifteen millions of years. This is certainly very slow cooling.

There is no need of our worrying about the earth and what is going on because we really can do little or nothing about it. Let us be philosophical and be thankful that we have many earthquakes instead of a few, that the sun shines and causes evaporation, that the rain falls and erosion and sedimentation take place. Let us not worry for fear the sites of our homes will be under the ocean some hundred million or so years from now. Surely any change in the configuration of the earth's surface will occur so slowly that the inhabitants of the earth will have time to move out of the way of anything that might cause them trouble.

THE GROWTH CURVE OF A SCIENTIFIC LITERATURE

NITROGEN FIXATION BY PLANTS[1]

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The Problem

One of the most obvious and yet almost neglected aspects of scientific research has been the accumulation of a technical literature. To label this literature "neglected" might appear to be a generous overstatement, in view of that constant demand for current periodicals and bound volumes in the majority of our scientific libraries, a demand that gives rise to that most irritating statement, "The journal you want is out." Appreciation of the literature, however, is confined primarily to its use as an additional tool for more research—it serves as a fountain of information and indicates guide-posts for present problems. While practically every scientist claims acquaintanceship with the literature of his particular field, his knowledge is usually restricted to the factual material contained therein. Study of the literature as an entity is almost completely unknown; its function in science is primarily that of a technical accessory—its meaning for the philosophy of research is little more than that furnished by gelatine plates, guinea pigs or graph paper.

It appears probable that this limited view of the function of publications might decidedly handicap the service of literature to general problems of science, however satisfactorily it meets the demands for mere technical assistance. At present, with the values of science seriously questioned by layman and official, an inventory of past and present activities is indicated to answer the recurrent questions of "Whither Science? Why? and How Far?" It is suggested that a study of the *biological properties* of the literature of various fields might provide one method of attack for the necessary inventory. By biological properties is meant the growth and development of a literature, especially as these are influenced by and reflected in the environment; subject-matter as well as size should be considered in evaluation of the data.

A census-taking of the publications in a given field should provide valuable information for the interpretation of past production and might afford some basis for prediction of future trends. Possible uses of an analysis based on the counting of heads among published papers would include the following:

(1) Provides an insight into the nature of the dependence of interest in a given problem upon external factors of an economical, political or emotional nature.

(2) Gives to the novitiate in a field an idea of the volume and distribution of the literature which forms the organic body of data to which he hopes to add.

(3) Enables a limited prediction with respect to future production of both number and content of papers. Awareness of future trends in production might allow a measure of control if this is deemed wise or necessary.

In order to lend itself to an analysis such as that proposed, the body of relevant material should possess certain characteristics. For example, interest in the subject-matter of the literature to be examined should have extended over a number of years. This insures that a

[1] Herman Frasch Foundation in Agricultural Chemistry, Paper No. 88.

more or less steady development rather than the mushroom type of growth has occurred. Likewise, the subject-matter should cover a fairly large but definite problem. The field should not be too sharply delimited; otherwise the volume of publications will not be sufficiently large to allow statistical treatment. On the other hand, unless the papers are concerned with a perfectly definite problem, the growth curve will represent the sum of a number of interlocking developments and as a result may defy rational analysis. Finally, the field of study represented by the literature being examined should be coherent and stable. If this is so, the research at a given time is intimately connected with past work and does not depend on the whim of the fad type of scientist.

Method of Attack

A body of literature that apparently satisfies the foregoing requirements is that which deals with the subject of nitrogen fixation by plants, especially by the *Leguminosae*. Because of the practical application, the literature on this subject boasts of an ancient and respectable lineage. A Chinese writer of the fifth century B.C. describes the use of the mung bean for green manuring. Reference to leguminous plants, likewise, are found in Egyptian writings of the pre-Christian era. Greek and Roman writers comment, often quite astutely, upon the importance and function of the *Leguminosae* in agricultural art. When in the early nineteenth century, agriculture was definitely regarded as a suitable field of inquiry for natural scientists, foremost among the contributions to the laws of husbandry were those concerned with the use of leguminous crops.

Albrecht Thaer, the foremost agriculturist of his day, strongly advocated the inclusion of the *Leguminosae* in crop rotation to the point of advising rotations in which leguminous crops predominated. So great was his influence that in 1856 he can state, "latterly the practice of sowing white clover with the last crop has become very general; only a few apathetic and indolent agriculturists, or men who are firmly wedded to old opinions and customs neglect this practice." Schultz-Lupitz, "the father of modern green manuring," urged the turning under of clover, lupines and peas for soil improvement. In America as early as 1801 Bordley writes in his "Essays and Notes on Husbandry and Rural Affairs": "Clover plowed in, together with the remaining of grain stubble, year after year will gradually meliorate the soil."

In light of these practical observations concerning the value of leguminous crops as enrichers of the soil, it is not surprising that the earliest researches of agricultural experiment stations deal with an examination of leguminous plants, especially with their nitrogen nutrition. In Europe as well as in America, the history of both federal and state experiment station is intimately connected with studies on the various problems associated with the question of fixation of atmospheric nitrogen by plants. The literature resulting from these researches has been reviewed by Fred, Baldwin and McCoy,[2] who in an effort to examine all important publications on this subject collected a file of over 2,000 references. Access to such a population arouses an irrepressible urge in the student of statistical science to make some sort of classification of the catalogued items. The problem then reduces itself to selection of a basis of classification which will provide the maximum information with respect to the biological properties of the population.

[2] Wisconsin Studies in Science No. 5, 1932. The authors wish to express their appreciation to Dean Baldwin and Professor McCoy for their cooperation in the preparation of this manuscript.

Before discussing the data, it might be well to consider briefly the methods used and to indicate certain limitations in the sample. The publications of a given year were divided into nationalities with respect to *total number* and to *total pages*. By nationality is understood that of the journal publishing the paper rather than the native country of the author, since the latter is frequently difficult to determine. In a large sample, heterogeneous "crossings," *i.e.*, Frenchmen publishing in German journals, should tend to compensate one another. In the case of papers appearing in journals which publish in several languages, *e.g.*, *Zentralblatt für Bakteriologie*, the publication was credited to that nation whose language was used. In the latter case it was necessary to ascertain the nationality of the author of those papers published in English. In order to eliminate, at least in part, the variations which arise from time lag in the date of publication after receipt of the manuscript, the data were averaged for a two-year period.

It might appear to be advantageous and desirable to classify the material according to subject-matter. This plan was rejected when it was found that many publications would fall into several groups with a resulting confusion that would defy analysis. Instead, the predominant topics of investigation of a given period will be discussed briefly in a general way in the text.

Although the authors of the monograph endeavored to collect all extant original contributions to the subject, it is obvious that certain papers were overlooked or eliminated from consideration. These will include: (1) A vast number of popular articles and semi-popular bulletins, the scientific nature and value of which are open to question; (2) papers of recent publication and hence not yet in the "literature," *i.e.*, abstracted or referred to by other authors; (3) papers appearing in obscure journals that are not abstracted; (4) papers with practical value for a given locality and whose circulation is accordingly limited, *e.g.*, station bulletins and reports. Cases three and four will tend to create a bias in favor of the United States, since very rarely would a publication on this subject in the United States fail to be noted in the *Experiment Station Record*.

THE EMBRYONIC PERIOD—BEFORE 1886

Production of research articles since 1884 pertinent to the subject of nitrogen fixation by leguminous plants is indicated in Fig. 1. Prior to the middle of the nineteenth century, a few papers had appeared on this subject, notably the studies of Boussingault, Ville and Liebig dealing with whether or not plants in general can utilize the free nitrogen of the air. From 1850 to 1884 this subject received increasing attention, a total of 90 papers appearing during this period. The contributions came from many countries and the rate from any one nation was erratic, hence no attempt was made in this figure to plot the growth prior to 1884.

The data were conflicting, the publications were often polemical in nature, and the question of the ability of plants to fix atmospheric nitrogen remained unsettled. It is interesting to note that in many of these papers the answer to the perplexing problem was shrieking to make itself heard above the din of controversy. *A posteriori* it is easy to see that Ville, Lawes and Gilbert, Boussingault and Atwater all published experiments which might have clarified the muddled situation, had the proper interpretation been forthcoming.

Ville in France claimed that all plants—legumes, grains and grasses—can utilize free nitrogen and convinced a referee board including such distin-

guished scientists as Dumas, the chemist, and Chevreul and Regnault, the physicists, of the truth of his claims. Ville's fellow countryman, Bossingault, likewise presented data from field experiments which had been carried out for 16 years and which seemingly offered strong proof that leguminous plants fixed atmospheric nitrogen. Liebig convinced the majority of investigators, including Boussingault himself, that the observed gains in nitrogen were only apparent and rested on errors in the analyses of the manure added to the fields.

Lawes, Gilbert and Pugh at Rothamsted in England conducted experiments which were masterpieces of technique and concluded that no plant is able to use free nitrogen. Cereals and leguminous plants were grown under glass in sterilized soil and supplied with sterile water and air; their exact control kept out the bacteria necessary for nitrogen fixation by the *Leguminosae*. These workers were cautious enough to suggest that the conditions of their experiments were artificial and abnormal, since field experiments indicated that *Leguminosae* obtained more nitrogen than was present in soil, rain and seed.

Probably closer to the solution of the problem than any of these was Atwater, of Wesleyan University, Middletown, Connecticut. In the early eighties, through "the generosity of Hon. J. W. Alsop, M.D., who, by defraying the larger part of the pecuniary cost and by aid in other ways, has made the investigation practicable," Atwater and his assistant, Woods, made rather extensive experiments with the pea plant. The plants were grown in the greenhouse and in the open in the presence of varying levels of nutrients, including combined nitrogen. The nitrogen balance at the end of the experiments showed conclusively that nitrogen had been acquired from the air in such quantities as to preclude the conclusion that it had been assimilated as ammonia. In spite of the fact "that plants assimilate free nitrogen is contrary to the general belief and the results of the best investigators on the subject," Atwater clung persistently to the belief that leguminous plants could use free nitrogen. In his first report (1884) before the British Association for the Advancement of Science, Atwater offered no explanation for his data; later in the *American Chemical Journal* he attempted to reconcile his findings with those of the "best investigators." Two factors that might have influenced his results and not those of the others were suggested—electricity and microorganisms. Both of these were novelties in science and Atwater appeared to embrace both without favor or prejudice as the possible missing link. In 1886 he again discussed his experiments in the December issue of the *American Chemical Journal*, stating:

> To what extent the attested acquisition of atmospheric nitrogen is a function of plant alone, in how far it may be dependent upon the action of electricity, microbes or other agency induced by the plant, by what species of plants and under what conditions it is accomplished are of course matters for future study. . . .
>
> I am aware of no observed fact to imply that these (ignited sand and nutrient solution) separately or together are able to fix nitrogen by aid of electricity, microorganisms, or any other means. In the present state of our knowledge, therefore the balance of probability seems to decidedly favor the assumption that the plants themselves must be factors in the acquisition of atmospheric nitrogen. . . .
>
> But whatever may be the plants that acquire atmospheric nitrogen, the ways by which they acquire it or the form in which it comes, the fact of its acquisition seems well established.

Atwater clearly recognized the possibility that both plants and bacteria may be factors in the fixation process but was confused by the other alternatives, notably electricity, and thereby missed the solution of the problem. Prophetically he adds:

> And late research leads us to hope that the explanation of the process by which the nitro-

gen is acquired may be found, perhaps in the near future.

The hope had already been realized. In 1883, Hellriegel and Wilfarth, two rather obscure German plant chemists, had begun a series of experiments which were destined to supply the key to the puzzle. Initially they had planned only to study the influence of the quantity of combined nitrogen on the yield of crops, seemingly a somewhat obvious and simple problem. At the end of two years of work, Hellriegel and Wilfarth could enumerate three facts, well established but unexplained. Cereal crops grown in sand cultures, supplied with a nutrient solution complete except for the element nitrogen, grew in proportion to the quantity of combined nitrogen added, but the growth of a leguminous crop apparently was independent of added nitrate. Moreover, chemical analyses demonstrated that the leguminous plant, peas, often showed gains in nitrogen which must have come from a source other than the sand or nutrients; no such gains were observed with the cereals. Finally, the pea plants in the *control* pots, *i.e.*, pots to which no nitrate was added, developed in a most confusing and inconsistent manner. Duplicate pots would be quite at variance with each other; the peas in one would be tall with dark-green foliage, in the other, stunted and yellow.

In a manner not unlike that of the detective in mystery fiction, these two methodical German investigators searched for a clue in the mass of evidence which would lead to a rational explanation of the seemingly irrational data. Four hypotheses, advanced by other workers to explain the differential response of cereals and leguminous plants to nitrogen nutrition, were carefully applied to the data, and each in turn rejected. Elimination of these hypotheses was in the main due to the unexplained vagaries of the control pots; four "beautiful hypotheses were slain by one ugly fact." But what at first appeared to be only a major tragedy of science proved to be in the end the long-sought-for clue. The curious, variable behavior of the peas in the control cultures could be interpreted as the random distribution of some unknown factor, a factor which entered the cultures from the outside and which enabled leguminous plants to fix nitrogen. If this were the case, one would expect the fixation to be unpredictable, since whether or not this factor reached a given culture might well be governed by the laws of chance. To construct an adequate hypothesis on the basis of this idea was the next step toward the solution. Hellriegel knew that the air contained microorganisms, that certain species of microorganisms were able to fix atmospheric nitrogen and that some plants lived symbiotically with lower fungi (mycorrhiza). Reports in the literature had offered strong evidence that the protuberances on the roots of the *Leguminosae,* the nodules, were filled with bacteria. These observations were combined into a simple yet complete explanation of the facts: bacteria from the air fell into the open sand cultures, infected the peas, forming nodules which enabled the plant to use free nitrogen; the bacteria were without effect on the cereal crops. Although this hypothesis brought order into the confused results of previous experiments, publication was delayed until it could be critically tested.

In March, 1886, Hellriegel and Wilfarth began the crucial experiment. In one series, seeds, sand, pots and nutrient solution were carefully sterilized and the sand protected from chance infection by sterile cotton; the second series was identical, except that the pots were deliberately infected with an extract of a rich garden soil; in the third series nothing was sterilized and infection left to chance. This experiment was a complete failure—in their anxiety to eliminate microorganisms a sand was obtained which had been strongly ignited; the ignition had formed an ash so alkaline

that none of the plants grew. The experiment was discarded and a second started in May; Hellriegel's sole comment on what must have been a bitter disappointment was that the duplicate experiment was begun "late, but as the following will show, not too late." Seldom has a critical test given such perfect results. Peas grown in sterilized sand behaved as did the cereals, *viz.*, the growth was dependent on added nitrate, but in the sterilized sand plus soil extract all cultures of peas grew luxuriantly and fixed quantities of free nitrogen. In the non-sterile sand the development of the peas was erratic, as in the former experiments. At harvest the verification was complete; peas grown in the sterilized sand were free of nodules, but those in the sand plus soil extract were covered with the characteristic tubercules. In the non-sterile series only those plants which were tall and thrifty were infected.

Hellriegel appeared before the agricultural experiments section of the Versammlung Deutscher Naturforscher und Aertze in September, 1886, dramatically exhibited typical pots of leguminous plants and cereals and made his report. His discussion of the results was brief, but the demonstration of actual plant cultures met all arguments. Ironically, this meeting in Berlin was presided over by Henry Gilbert of Rothamsted, whose painstaking experiments of thirty years previous were thought to have settled the question, but whose conclusions were now shown to be in error—an error arising from too much care! Undiscouraged, Gilbert returned to England and soon published an account of experiments which are masterpieces of technique and which completely confirmed the findings of Hellriegel and Wilfarth.

The Period of Development (1886–1914)

The discovery of Hellriegel and Wilfarth immediately stimulated research on nitrogen fixation; reference to the figure shows that a real bull market resulted during the next few years. Scientists of many nations vied with one another in turning out copy at a terrific pace. Germany dominated the field, but France was also prolific; the British Empire produced at a more conservative rate and America made modest contributions through the efforts of Atwater and collaborators. Many of the papers during this period of accelerated development were less than ten pages in length and were often published in series. An author would publish six or seven articles bearing the same title in a single volume of a journal, each article dealing with a detail of one major experiment. It is probable that the modern editor would not accept such "squibs," at least on this subject, and this must be kept in mind when comparing the output during this Golden Age of production with later periods. Subject-matter of most of the contributions is monotonously similar—the experiments of Hellriegel and Wilfarth have been confirmed. It is a tribute to the genius and careful work of these two men that few dissenting votes are noted among the many checks on their work.

Interest in the question of whether or not all plants can use free nitrogen was revived, and during the period 1890–1895 the last mass attack was made on this question. The old experiments were repeated and new ones devised, but the results with plants other than members of the *Leguminosae* were uniformly negative. Since 1900 investigations on this subject have dwindled to the sporadic efforts of the more intrepid workers.

One important contribution made during this period must not be overlooked, *viz.*, the isolation in pure culture of the causal organism. Hellriegel and Wilfarth were plant chemists and were uninterested in the finer points of the biology inherent in their work. They employed for inoculation crude culture

of soil or extracts of soil. In 1888 Beijerinck isolated pure cultures of the organism from the nodules of various leguminous plants and laid the foundations for the extensive bacteriological studies necessary for final solution of the problem.

About 1895 the zeal to verify the experiments of Hellriegel and Wilfarth had largely subsided and the rate of production fell off decidedly. Simultaneously the content of the papers changed; editors probably began to reject what an unkind critic might term "pot-boilers," and attack on some of the more complex features of the symbiosis became necessary. With the formulation of the complicated aspects of the problem, there was a sudden loss of interest which caused a sharp dip in the curve of production. Nevertheless, during the period of this "low," some of the most suggestive and provocative contributions were made, notably by the Germans, Frank, Nobbe and Hiltner, and by the Hollander, Beijerinck. A few of the questions raised by these investigators, many of which have not received final solution after 40 years of research, include:

(1) Does one species of bacteria attack all species of *Leguminosae*, or does each species of plant require a special type of bacteria?

(2) Are the bacteria capable of fixing nitrogen apart from the proper host plant?

(3) What is the explanation of strain variation, *i.e.*, why does one strain of the organism benefit the plant, whereas another strain of the same species proves non-beneficial?

(4) What is the best manner to apply artificial inoculation to crops? Under what conditions should artificial inoculation be used?

(5) What is the chemistry of the process?

(6) What members of the *Leguminosae* are especially suited for use as green manures? How should these be handled?

(7) What is the order of magnitude of nitrogen returned to field by various leguminous crops when used as a green manure? When the crop is removed?

From 1895 until 1915 there is a gradual but steady increase in the rate of production of research papers. In these 20 years the influence of German science is manifest; without a doubt, the Germans were the leaders in this field with respect to both quality and quantity of contributions. At the beginning of the century the number of papers by American investigators rose rapidly and soon were threatening the German dominance. Part of this was due to the efforts of Moore at the U. S. Department of Agriculture, who succeeded in popularizing this field of research among the investigators at experiment stations. Likewise the expansion of experiment stations in numerous states aided in swelling the total of publications from the United States. A large number of the American contributions at this time were in the form of station bulletins and dealt primarily with practical problems, *e.g.*, the type of leguminous plants to use in a given locality or with the benefits of artificial inoculation. While the publications from the United States increased rapidly, those from the British Empire exhibited a more restrained development and those from France declined. It is noteworthy that research on this problem in countries other than the "Big Four" was definitely established during the period from 1895 to 1915.

Approaching Maturity (1915—)

Even before the world war, there was evidence that the United States was to assume leadership in the production of research concerned with nitrogen fixation by the *Leguminosae*. With the start of the conflict this occurred. Among the European nations the quantity of research fell off sharply during the period from 1914 to 1919, while an actual expansion took place in the United States. The explanation of the increase in the productivity of America during the war probably rests on the fact that food production for man and beast was the feature of our participation. Research in the field under con-

sideration accordingly would be stimulated by the national crisis; the increase does not imply non-participation in war activities by the American scientists as might be superficially inferred.

One of the most important of the practical contributions made during this period was the development of reliable methods for supplying pure cultures of the bacteria for artificial inoculation of leguminous seed. Such cultures made unnecessary the use of the laborious and haphazard soil transfer method previously used to provide the proper organisms. The majority of previous attempts to furnish pure cultures had proved disappointing and frequently disastrous mainly because lurid, exaggerated claims made to sell the cultures were substituted for scientific control in their production. Research at several experiment stations, notably the Soil Bureau at the U. S. Department of Agriculture, Wisconsin and Cornell, resulted in the development of a technique which insured successful production of dependable cultures of the various legume bacteria. Because of the previous exploitation by unscrupulous purveyors of "inoculum," farmers viewed with skepticism if not suspicion the new product. Gradually through demonstration and education they were not only convinced of the advantages of artificial inoculation for leguminous crops but also appreciated the limits of the benefits to be expected under various conditions.

At the termination of the war, the contributions from the United States showed a definite decrease, a reaction common to many of the wartime industries. This decrease continued into the early twenties, but at the same time research was revived in Europe so that the curve of production for the entire world began to rise. About the period from 1922 to 1923 the rate of production climbed in all countries, but especially in the States. The upturn in this country was so precipitous that about 1928 there is evident a sharp peak in the curve of production for the United States, a peak that carried the curve for the entire world into dizzy heights. This period of pronounced inflation is rather disconcerting to one endeavoring to find law and order in the growth of a literature, but it is believed the following explanation accounts for the sensational rise and subsequent fall.

In all countries, a political philosophy of self-sufficiency gained varying degrees of support. This political ideal of ability to provide all necessities within the national border would certainly focus attention on the fact that an intensive agriculture depends on a supply of cheap nitrogen. As an organism seeks to make the correct response to a changed environment, so would the state seek methods to furnish this requirement for a nationalistic existence. Under these circumstances impetus to research concerned with fixation of atmospheric nitrogen by biological or artificial means would be expected. Another factor that probably contributed to the rapid development during the twenties was the preeminent position of all natural sciences. Scientific achievements of the war had so appealed to the mass mind that they turned hopefully to the scientists for the means to rescue them from the complex political, social and economical difficulties which ushered in peace. Popular support for all research was to be had even in those countries whose financial status was precarious. Later, disappointed with scientific leadership, the mood changed and scientific programs suffered deflation.

In America the factor of economic prosperity, absent in other nations, undoubtedly played a rôle. One result of this prosperity was an expansion in the budgets of institutions devoted to research—it is probable that many experiment stations and universities undertook work in fields that ordinarily would be classed as "marginal." With the onset

of the depression came tightened purse-strings and administrative frowns on research concerned with production aspects of national economy. The effect on the literature curve of nitrogen fixation by leguminous plants is unmistakable; the similarity of the curve with that of 50 representative stocks on the New York exchange for the period from 1923 to 1933 is readily apparent.

Although the caliber of much of the work reported during the period of prosperity must await future evaluation, the stimulus to production resulted in at least one desirable feature, *viz.*, the broadening of research programs. The practical problems which dominated the war period were stressed comparatively less and some of the more fundamental aspects were attacked. These included a complete reinvestigation of the strain variation problem and of nitrogen fixation by the bacteria apart from the host plant; new fields tentatively explored were detailed studies on the biology of the organism and on the biochemistry of the fixation process.

As an example of the manner in which theoretical studies often lead to conclusions of great practical significance may be cited the progress of the so-called "strain variation" problem. About 1920, work was started at the Wisconsin experiment station on a fundamental study of physiological differences in strains of the organisms isolated from different plants of the same cross-inoculation group. The research eventually demonstrated that mere inoculation with organisms of the proper group did not insure benefit to the host plant, since ability of different strains to fix nitrogen after invasion of the plant varied widely, some strains being definitely parasitic on the plant. This fact provides added reason for the use of pure cultures of known efficiency in order to insure maximum returns and at the same time emphasizes the need for exact control in the distribution of artificial inoculation.

The discussion has been restricted to *number* of papers which may not be the best measure of production. Because of differences in editorial policies or in methods of publication, the size of papers in various countries may vary considerably, hence the number of papers per year may be a fictitious criterion of research capacity. A plot was made of the mean size of papers since 1883; the data showed that on the average 60 per cent. of the papers are less than 10 pages in length, 20 per cent. are from 10 to 25 pages; from 10 to 15 per cent. are from 25 to 50, and the remainder are over 50 pages. The percentages did not vary considerably over a period of 50 years, although a preponderance of small papers featured production during the decade from 1885 to 1895. Since many of these were from Germany, a plot similar to Fig. 1 was made in which the output in number of pages per nationality was used as the ordinate. The resulting graph was little different from that given in Fig. 1; the peak during the period from 1885 to 1895 was not quite so pronounced and the German dominance from 1885 to 1910 was less marked.

The Extended Population Curve

The treatment so far has been a more or less empirical description of the growth of a body of literature together with observations regarding possible forces which influenced the growth. It is of interest to determine whether or not a quantitative expression of this development can be obtained. The mathematical curve that might be expected to describe the growth would be a representative of the logistic, or population, family. These curves describe phenomena which behave in the following manner: (1) The rate of production at a given time is proportional to total production (or change in rate of production is proportional to the latter); (2)

there is some upper limit to the rate of production.

A priori these considerations would appear to apply to the development of the literature of a given research, *i.e.*, the production during a given year should be roughly proportional to the total produced, but a limited rate of production would be eventually reached. The first of these follows from the fact that the more publications which appear, the larger is the audience that is likely to become interested in the problem; likewise the tendency to follow the crowd is evident, even among scientists. The second condition, *i.e.*, the attainment of a limit, takes place when the field becomes saturated; new contributors are no longer readily attracted because of either dearth of problems or difficulties in publication.

Mathematically[2] these conditions are expressed:

$$\frac{d^2N}{dt^2} = K\frac{dN}{dt}\left(A - \frac{dN}{dt}\right) \text{ which integrates into}$$

$$\frac{dN}{dt} = A - \frac{A\,(A-\alpha)}{A - \alpha + \alpha\, e^{kat}}$$

A—limiting rate of production
N—total number at time t
α—constant representing production at $t = 0$
K—proportionality constant

Tamiya[2] has shown that the literature devoted to research on the fungi *Aspergillus* follows quite closely a curve of this type.

The data of Fig. 1 were replotted and by trial and error the constants evaluated for the arbitrary origin t equals 0 at 1860 and α equals unity. Since even a casual inspection shows that in no case will the fit of the points be close to a smooth logistic curve, no attempt was made to secure the "best" curve in the least square sense. The population curve that appears to best fit the data is shown in Fig. 2. It is apparent from a study of this figure that the points follow the indicated curve in a general way, but that the fit leaves much to be desired. The worst departures from "theory" occur at: (1) Following Hellriegel and Wilfarth experiment; (2) the world war; (3) 1928–1929. The explanation of these peaks and valleys has been already indicated.

[2] *Botanical Magazine*, 45: No. 530, 1931.

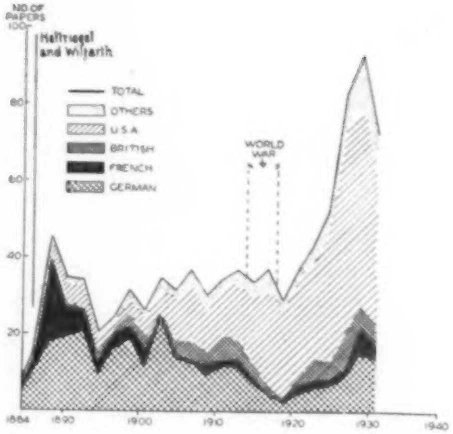

FIG. 1. THE DISTRIBUTION OF PUBLICATIONS ON NITROGEN FIXATION BY *Leguminosae* SINCE 1884.

It appears that the subject of nitrogen fixation by leguminous plants is so intimately connected with the business of living that the research in this field is rather sensitive to upheavals in the political and economical world. The subject-matter is so closely intertwined with the larger problem of production that support of research at a given period is a function of current economic

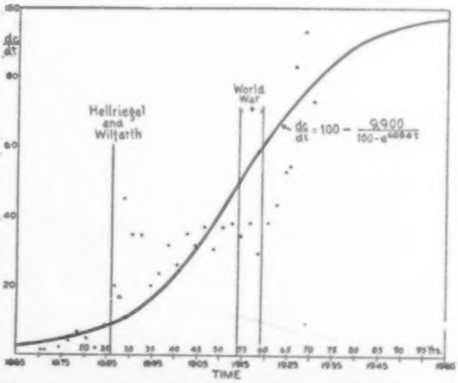

FIG. 2. GROWTH CURVE OF THE LITERATURE ON NITROGEN FIXATION BY LEGUMINOUS PLANTS.

or political theories as well as the "natural" inclination of a given scientist to work in this field. In the theoretical development of the equation it was postulated that this inclination was conditioned only by magnitude of previous work and by proximity to the ultimate limit. Because of the effect of superimposition of external on the internal factors governing development, the growth appears to be more lawless than is really the case.

On the other hand, the literature of research with *Aspergillus*, an organism used mainly in physiological and anatomical studies, reflects academic interest primarily and is much less responsive to affairs of the world outside the laboratory. In such a case the fit of the observed with the "theoretical" might be fairly close, as was actually found.

Another influence which has caused occasional departures from the smooth curve in the case of the literature of nitrogen fixation is the ability of certain leaders to stimulate research. Attention has been called to Hellriegel and Wilfarth in Germany and to Atwater and later Moore in the United States. In recent years Virtanen of Finland has done much to accelerate the research in eastern Europe, with the result that the work of *Others* has become an appreciable part of the total. However, the impetus given to research in the Union of Socialist Soviet Republics since the revolution, a political event, has also been a factor in increasing the output of the unclassified nations.

In conclusion, it appears from the "smoothed" data that the research student of the future can look forward to an annual production of approximately 100 publications a year in this field. This limit of production seems likely to occur about 1965 to 1970. Likewise, the total number of pages to be mastered each year will be from 1,500 to 1,600 before the harassed student may look for relief from an ever-increasing annual load.

It is highly desirable to apply proper correction factors to the foregoing estimates in order to compensate for the apparent powerful influences on the research exerted by wars, depressions, revolutions and individual leadership. Until the effect of the external environment can be mathematically expressed in our equation of growth, at least as a first approximation, predictions based on a theoretical curve may or may not be more reliable than those found in a daily racing form.

WILLIAM HENRY WELCH, SCIENTIST AND HUMANIST

By Dr. DAVID RIESMAN

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In my life as a doctor I have met three men and only three who exercised a personal influence of such magnitude that they dominated the medicine of their time. These three men were Osler, Billings and Welch. Differing in mental endowment and in ancestral background, each had a personality that separated him from his fellow men. Osler typified what is still comparatively rare in America, the scholar in medicine. To him is largely due the growing interest in the humanities among medical men, although that is not his only claim to fame. He pointed out in his writings and in his teaching the value as well as the charm of clinical or bedside study. Throughout the land his younger contemporaries and his successors have striven to emulate him as a teacher, to be second Oslers. Frank Billings was not a profound scholar. The humanities attracted him but little. With the magnificent frame of a pioneer in a pioneer region he had time mainly for the practical things of life and left the ornamental to others. Forthright, clear-thinking, generous, a first-rate doctor and far-visioned educator and organizer, he acquired an extraordinary influence through the length and breadth of the land. His direct personal influence on the host of young men who came within the range of his personality made devoted disciples of them. They became Billings men, keen clinicians, able diagnosticians and safe counselors in the crises of life.

Welch did not come from a raw and sparsely settled country, like Osler, nor from the striving and restless Middle West, like Billings; he was the product of the best stock in an old and long-settled community, a stock that has impressed itself upon our nation as no other. In the little town of Norfolk, Connecticut, and in the immediate neighborhood the Welches had lived for generations, mainly following the profession of medicine. He himself was the tenth Dr. Welch in the family. His father, William Wickham Welch, a beloved practitioner, was to the old citizens of Norfolk the most famous of the entire Welch line. To him his son William erected a fountain with the beautiful legend, "Fons sum solati, talis et ipse fuit—I am the Fountain of Comfort as He was Himself." From such an ancestry, in which culture and self-assurance had been at home for generations, Welch inherited the personal dignity and the charm of bearing that were his striking possession. He was at home in any circle—the great men of capital and business, statesmen, the highest military officers, even Presidents were proud to be his friends. His figure was not a commanding one—he was short, stocky, thick-necked and obese; nevertheless, he was the focal point in any gathering, medical or nonmedical.

Welch had the gift of friendship in a rare degree. Courteous, kindly, tolerant, not given to sarcasm, free from the superiority complex, to which more than any one else he was entitled, he had no enemies. In some men that might signify a weak character, one unwilling to assert itself. That was not true of Welch. At scientific meetings he never hesitated to criticize a speaker with whom he did not agree, but he always did it with such fine tact, with such good nature that he did not make an enemy

of the man he criticized. At the banquet given in his honor in 1910, Councilman said, "Has any man ever heard Welch speak ill of his neighbor?"

Welch was born in 1850 and was sent to Yale College at the early age of sixteen. At that time the classics still dominated education, so that, together with the sciences, he acquired a knowledge of Latin and Greek which he increased by teaching these languages for a year after graduation. It can perhaps not be proved, but personally I feel that the type of education he received—largely humanistic—made it possible for him to become the all-round scholar that he was. I am not reconciled, and I doubt whether Welch was reconciled, to the elimination of the so-called dead languages from the college curriculum. In truth, they are not really dead, for they are still the sources of the language of science. To Welch Latin and Greek were more than two more languages. They were the key to two more civilizations. It is, I agree, no longer necessary to give them the main place in the college curriculum. Even so sturdy a defender of Greek as DuBois Reymond, after having first pleaded for Greek as a requirement for a medical education, late in life cried out, "more conic sections and less Greek."

After teaching Latin and Greek for a year Welch entered the College of Physicians and Surgeons in New York City. With prophetic vision he soon saw the value and need of chemistry, and this induced him to return to New Haven for advanced studies in that and kindred fields. He took courses both in the Sheffield Scientific School and in the Yale Medical School and was greatly stimulated by Professor Oscar Allen and by George Frederick Barker, afterwards professor of physics in the University of Pennsylvania. Welch became deeply interested in the new chemical theories propounded by Kekulé, the originator of the benzol ring concept of organic compounds. Within a year he had mastered the exciting theories in the original German. On completing these important studies Welch in 1872 re-entered the College of Physicians and Surgeons, where the course of instruction had recently been extended from two terms of four months each to three terms of five months each. Through the influence of Francis Delafield, one of the founders of pathology in this country, Welch began even as an undergraduate to make autopsies at Bellevue Hospital and to study morbid changes with the microscope. He had won the Seguin Prize, a Varick microscope, for the best report of the clinical and didactic lectures of Seguin, famous professor of nervous diseases. His graduation thesis on goiter not only won for him the first prize, but also in its preparation taught him the use and value of medical bibliography.

Six months before graduation Welch was already acting as intern in Bellevue Hospital. Here he came in contact with a brilliant group of men such as Francis Delafield, E. G. Janeway and Abraham Jacobi. A number of others also influenced him at this formative period—Charles McBurney, Alonzo Clark, Austin Flint and Alfred Loomis. At the end of a year and a half's internship Welch, accompanied by his friend, Frederick S. Dennis, set sail for Europe. For him and for American medicine this proved to be a momentous step. The Civil War had interrupted the migration to Europe, which in the earlier decades of the century had taken some of the best men to Paris, London and Edinburgh. Germany was just beginning the wonderful work in medicine, in chemistry, in biology that eventually made her the Mecca for eager learners from all parts of the world, a position she lost with other intangible things as the result of the great war. Strassburg, which the Germans had raised to a university of highest rank, was Welch's

first stopping place. Here he worked with Waldeyer in histology, with Hoppe-Seyler and Baumaun in biochemistry, virtually a new science at that time, and with von Recklinghausen in pathology. After a short stay at Strassburg he went to Leipzig, where he found many other foreigners in the famous physiological laboratory of Carl Ludwig. Magendie, Charles Bell, Johannes Müller, Claude Bernard in the earlier part of the century had blazed the way that was now being followed with brilliant results in physiology by Helmholtz, by DuBois Reymond and by Ludwig. Welch discussed with Ludwig the advisability of working with the mighty Virchow,[1] but Ludwig, who was not completely won over to the cell-doctrine, suggested that Welch instead go to the youthful Julius Cohnheim in Breslau, which was a happy choice and proved of decided importance for Welch's future. As we shall see later, it was Cohnheim's recommendation that largely influenced President Gilman of Johns Hopkins University to offer the chair of pathology in the new medical school to Welch.

In Cohnheim's laboratory Welch was associated with Karl Weigert, to whom pathology owes many important discoveries, including valuable methods of staining tissue elements in preparation for microscopic study. Others in Cohnheim's remarkable institute were Ehrlich, Lassar, Neisser and Salomonson. The atmosphere was electric. Every one was pursuing some individual problem assigned to him by the master. That of Welch was the ascertaining of the causes of acute general edema of the lungs. The result appeared in an essay that is still a classic; it was written by Welch in German and printed in *Virchow's Archiv* essentially as Welch had prepared it. A number of years ago I had occasion to read this article. At that time I thought that some one had translated it from Welch's English into good German. It did not seem possible that any one not a native or long familiar with the German tongue could have written it in the vernacular.

[1] Nevertheless, Welch's admiration for Virchow was unlimited. He recognized in him the pioneer, the master—*il maestro di color che sanno*—and put him on a pedestal with Vesalius and Harvey.

Summer vacations in Europe were used by Welch for walking tours. During one short holiday he walked 210 miles in Switzerland. That was good exercise for a man inclined to be stout and gouty.

While at Breslau Welch had a memorable experience that helped to shape his subsequent career. He was present when Robert Koch arrived to show Cohnheim his bacteriologic discoveries, which consisted at that time chiefly in isolating and growing microorganisms—it was still five years before the discovery of the tubercle bacillus.

On Cohnheim's advice Welch went to Prague to visit Edwin Klebs, who was then engaged in his fruitful studies on acute endocarditis and diphtheria. From Prague he passed on to Vienna. He was disappointed in the opportunities he found there for the object he had in view, the study of embryology. Nevertheless, his stay there in the fall of 1877 proved invaluable. He entered with zest into the cultural activities, into the music and art life of the gay Austrian capital, the gayest then, as it is the saddest now. After the winter in Vienna he returned to Strassburg to continue his studies with von Recklinghausen, who was one of the leading representatives of the Virchow school of pathologists. As a suitable *Arbeit* von Recklinghausen assigned to Welch the study of the inflammation of the frog's cornea, induced by various caustic chemical agents. This study was intended to determine the origin of the pus cells, a question which at the time, the late 70's, was the cause of intense and bitter controversy between the Virchow and the Cohnheim schools. Was the pus cell

an emigrated leukocyte of the blood, as Cohnheim contended, or was it the product of the multiplication of the fixed tissue cells? Although Welch's experiments seemed to favor the latter, or Virchow theory, he was not willing to draw any far-reaching conclusions on the basis of his own researches. Indeed, it required many years of further observation and experimentation before the final proof was brought that connective tissue cells under the influence of chemical stimulants become motile and multiply so as to surround and to invade the chemically altered area.

His work at Strassburg being completed, Welch spent a few delightful days in Paris and then paid a second visit to London. Here he found the medical world in a fever of excitement over Joseph Lister's bold surgical exploit of opening the knee joint. We of to-day can hardly understand this excitement, unless we remember that antisepsis was still in its infancy.

His Wanderjahre being ended, Welch was ready to turn his steps homeward. He had studied under some of the best masters in the world. The phrase that I have just used is rarely heard in this country. We would say a man went to Pennsylvania or Harvard, to Yale, to Johns Hopkins, but in Germany a man studied under this or that professor. The distinction is significant. When our universities become real universities in fact as well as in name, then a man will study under such and such a master rather than at such and such a place.

Welch was back in New York in the spring of 1878, trying to see whether any one prepared as he was could find work in that great city. As he needed to earn a livelihood he was for a time undecided whether to stay in New York or whether to join his aging father in the practice of medicine at Norfolk. At that critical moment he fortunately fell in with Dr. Golthwaite, a well-known quiz master. The quiz was, as Welch long afterwards put it, a life-saver. But he abandoned the work after three years because he felt that it injured both the students and himself. The identical conclusion was reached by many of us in Philadelphia who as students and as teachers had experience with the quiz system.

During this probationary period in New York a little practice came to him from his old teacher Alonzo Clark, but it did not divert him from his main objective, the study and teaching of pathology. His Alma Mater offered him a lectureship in pathology, but as it did not provide a laboratory he with some heartache accepted the offer of the rival school, Bellevue Hospital Medical College, where three small rooms for the purposes of a laboratory were assigned to him. This constitutes in a sense an historic event, for it was the first recognition in this country of pathology as an independent discipline. Soon, however, the College of Physicians and Surgeons realized the need of a similar chair and offered it to Welch. He declined, feeling that he could not desert his friends at Bellevue and suggested T. Mitchell Prudden for the place. With the latter's prompt appointment pathology was definitely established as a branch of medical education coequal in importance with anatomy and physiology. To Virchow belongs the credit of having done this originally, for it was he who raised pathology to a definite science.

Welch's work in New York in teaching and investigation soon attracted attention. One day a man came into the room while Welch was lecturing on syphilis and without making himself known remained throughout the teaching period. The visit had momentous consequences, for the silent visitor, John S. Billings, had come to select a man to fill the chair of pathology at the new

hospital and the contemplated medical school in Baltimore. President Gilman, one of the greatest educators in the history of this country and one of the best judges of human nature, planned to create a medical school on new principles. Welch, recommended not only by Billings but also by the influential Julius Cohnheim, received the first appointment to the faculty. In that manner a pathologist became the head and real founder of the Johns Hopkins Medical School. Realizing the growing importance of bacteriology and his own need of knowing it in order to fit himself for his new position, Welch returned to Germany in the summer of 1884. After a year of intensive study he came back thoroughly at home in the newer bacteriologic methods of Robert Koch. He began his work at Johns Hopkins Hospital in 1885, several years before the medical school was opened. His energy and magnetism drew to him a group of men whose names are now household words, Councilman, Abbott, Nuttall, Halsted and later Flexner, Barker, Mall and MacCallum.

In the choice of the major faculty of the Johns Hopkins Medical School Welch exercised a paramount influence. It was through him that Halsted was called from New York and Osler and Kelly from Philadelphia. What that remarkable quartet meant for medicine in America I need not discuss. Its influence continues to this day after all but one of the four have died.

Welch was the dominant figure in the new medical school and rapidly extended its reputation far beyond the confines of Baltimore. Everywhere, in all medical schools, Welch's personality began to be felt. His influence upon medical education was manifold and in its totality was unquestionably greater than that of any other man in the history of this country. It is not easy to analyze it, for it was all-pervading and touched on many fields. One outstanding phase was the making of pathology a required fundamental branch in medical teaching. It was natural that in such an undertaking he should emphasize the value of the laboratory. As a result of this emphasis practically every medical school of importance and even many hospitals established pathological laboratories. Some were for research as well. What this meant for the progress of medical science in this country can not be over-estimated. Compared with Germany, the spirit of research was but poorly developed in this country when Welch established his laboratory in Baltimore.[2] He attracted to himself a group of eager young men who advanced so rapidly in reputation that they were called upon to fill chairs in other medical schools. In that way Welch's influence reached out over the length and breadth of the land. Councilman went to Harvard, Flexner to Pennsylvania, Lafleur to Montreal.

One reason for Welch's remarkable influence was that he better than any one else of his time was aware of the trend of medical education. With unparalleled vision, he sensed instinctively the direction in which medicine ought to go in order to progress, and thus it was that in time he became general educational consultant to medical schools, which rarely adopted a new idea or filled professorial chairs without seeking his advice. Welch was the foremost champion in the medical profession of the so-called full-time system of teaching. One may differ about the wisdom of full-time clinical chairs, at least on the plan originally conceived, but it can not be denied that the academic type of medical teaching is rapidly extending.

[2] A distinguished professor of physiology in a German university once said to Welch, "When America does wake up to the necessity of these things (medical laboratories) then let Europe look to its laurels."

In line with Welch's work for medical education were his efforts in promoting the founding of great research institutions, in particular the Rockefeller Foundation and the Rockefeller Institute, whose benefactions very nearly reconcile one to the accumulations of great wealth in single hands.

A large share of credit must also be given to him for the phenomenal physical improvement in medical schools, which until the early nineties for the most part were but poorly equipped. In 1890–91 there were only five endowed chairs in medical colleges and not a single one of these south or west of Philadelphia. On the other hand, there were 171 endowed chairs of theology, many of these being in the West and South. In 1892 the productive funds in the hands of the medical schools were $611,214, for theological schools $17,599,979. While it would be an exaggeration to say that the reversal of this relationship now obtaining was chiefly due to Welch, his powerful far-flung influence was always exercised in the direction of the physical and pedagogic improvement of medical schools.

As commissioner of health of Maryland, a position he held for many years, Welch set a standard for similar officials in other states; indeed, in all great events of national and state action for public health Welch was the man to whom every one turned for advice both in formulating plans and in selecting personnel. He laid down the lines of action in the great campaign against tuberculosis in this country, and they have been followed throughout the world.

Upon Philadelphia medicine Welch's influence was far greater than is generally known. The Phipps Institute, to whose board of trustees he belonged from the beginning, depended upon him for wise counsel and stimulus. He was consulted upon faculty appointments in the medical schools and upon other matters of educational policy. At the opening of the William Pepper Clinical Laboratory Welch delivered the principal address, which was a masterpiece of historical study of the origin and growth of laboratories.

When Welch reached the age of 66 he resigned his professorship of bacteriology at Johns Hopkins University, not, however, to seek the *otium cum dignitate* to which he was so well entitled; on the contrary, he took upon himself the directorship of the School of Hygiene and Public Health, a position he filled with undiminished ability for ten years. At the ripe age of 76 his friends may have expected that he would decide to retire, but Welch was not the man to rust out. Instead of seeking a life of leisure he began a work which in the future may prove almost as significant for American medicine as anything he did in his life. Supported by generous benefactors he created out of nothing an Institute of Medical History and the wonderful library bearing his name. After he was appointed to the new position he spent two years abroad gathering rare books in all parts of Europe. His unequaled bibliographic knowledge made it possible for him to collect a significant library such as was never collected before in so short a time. The institute, modeled after the famous Leipzig Institute of Professor Sudhoff, is destined under Welch's successor, Dr. Henry E. Sigerist, to play an important rôle in the cultural life of America.

The appointment of Welch to the directorship of the Institute of Medical History was not a mere gesture to a deserving man but a proper tribute to one who knew the history of medicine as well as any man in the field. William Osler once said, adding to a phrase of Oliver Wendell Holmes, "In addition to a three-story intellect Welch has an attic on top." In that attic was stored

a vast amount of cultural knowledge. And what was as remarkable as the contents of his multi-storied brain was the fact that on the spur of the moment he could call upon his memory for details that often gave the impression of careful, lengthy preparation. His memory was truly colossal. He could lecture for an hour or more on historical subjects without making a mistake in dates or sequence. I heard him give the first William Wood Gerhard address before the Pathological Society of Philadelphia. It was, appropriately to the occasion, on the history of typhoid fever. Without notes he began at the beginning, went through the early nineteenth century to Gerhard and then down to modern times without an error and without halting for a word. It was a phenomenal performance that no one present can ever forget.

When Welch reached his eightieth birthday a celebration of truly cosmic proportions took place. In Washington President Hoover attended, and speaking to the 1,600 assembled guests he called Dr. Welch "our greatest statesman in the field of public health." Nearly all large cities of the world held simultaneous celebrations. Welch's address on this occasion was a model of modesty and charm. One of the speakers said, "Dr. Welch waded knee-deep in honors unsought and aroused no shadow of envy or enmity on the way." To the truth of this all who knew Welch personally can testify. No man in medicine was ever so deep in the hearts of his contemporaries.

Welch's enormous influence upon his generation, indeed upon two generations, was due to a combination of gifts. First and foremost was that of good health, with its accompaniment of zeal and vigor, and the second curiosity; curiosity that was not satiated by the triumphs of medicine but extended to literature, to art and to music. His interest in the intangible things that we call the humanities kept pace with his scientific interest. With the scholarship of an Erasmus, the integrity and tolerance of a Contarini, the scientific zeal of a Virchow, we may, I believe, call him the finest exponent of humanism in the history of medicine.

Dr. Welch left no children in the flesh, a disappointment to the eugenist, but no man in the history of American medicine has left a larger number of spiritual offspring, a form of immortality, beyond theology and dogma, reserved only for the greatest of men.

If now, in conclusion, we place William H. Welch before our mental eye to see why he had such a colossal influence upon men and events, we find that he was an all-around personality—not only a doctor, not only a scientist, not only a historian, not only an educator, not only a scholar in literature and languages—he was all these at one and the same time.

Nature unfortunately is parsimonious in such men.

FOOD PROCESSING AND HUMAN WELFARE

By Dr. MARION DEYOE SWEETMAN

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In times of severe economic stresses and social tensions, the laboratory scientist has often disclaimed responsibility for these difficulties. He denies that his discoveries and inventions have produced any net increase in cultural maladjustments or, admitting that maladjustment has followed from application of his research, contends that inadequate social, economic and political leadership are at fault. Neither alibi survives the critical analysis given by Baldwin in a recent issue of THE SCIENTIFIC MONTHLY of the causes of technological unemployment. Natural scientists tend to minimize the complexity of social changes and consequently have a limited conception of the less immediate results of their contributions to human knowledge. Perhaps it would be a wholesome experience for every researcher to take time and thought to judge the welfare value of applications of the discoveries in his field. Such a belief has led to the following attempt to evaluate modern food processing.

It is neither necessary for us to wait upon years of experimentation nor to collect more records of the consequences of dietary habits to trace or predict some results of any dietary change with considerable accuracy. Almost within the bounds of this century, bacteriology, physiology and biochemistry have placed in the hands of the nutritionist exact and objective standards for judging the welfare value of foods. To evaluate any form of processing he has but to ask the following five questions, knowing that techniques for their answer lie at hand:

(1) Has an alteration in the nutritive value of the food been such that its use is not likely to render the average diet less well balanced?
(2) Has the product been altered significantly in digestibility?
(3) Is it equal to other forms of the same foods or similar foods in palatability?
(4) Is it wholesome, that is, free from harmful organisms and substances?
(5) Is it economical in terms of money, labor, fuel requirements and equivalent values in other forms?

Rating by such standards does not condemn all food processing. Scientific evaluations do not compel nutritionists to join the raw or the natural food evangelists. There is no sound reason for assuming that any combination of natural food materials, raw or cooked, will produce the best possible nutrition of any group of individuals under all environmental conditions. The only food designed solely for that purpose is milk, and it is now known that even the human product does not exclusively meet the infant's requirements for but a small fraction of the nursing period. In northern climates it must early be supplemented by such an "unnatural" addition as cod-liver oil if bone development is to proceed normally. No matter how teleological one may be in his attitude toward nature, nor how impressed he is by racial customs which seem to have had survival value, he must admit that nature's criterion of either survival or welfare is hardly acceptable to the modern human as an individual. Her chief concern is with racial continuity. Instead of being content with physical and mental vigor that suffice to meet biological needs and that persist through the reproductive period and such additional years as are of survival value to the children, individual man strives for more years of life

and extension of vitality for their enjoyment.

The golden age of human nutritional welfare, then, is not to be sought in the past, when foods were more or less unaltered plant and animal materials. It lies in the future, when man shall have learned to alter this part of his environment to attain as nearly as possible optimum health and vitality in any part of the earth he chooses to inhabit.

Furthermore, there is no validity in the assumption that household food processing is essentially superior to factory food processing. The nutritionist does not believe that dietetic welfare can be improved only by reinstatement of the domestic kitchen as woman's proper sphere, whether this is brought about by the coercion of a Mussolini or a Hitler, or by the persuasion of a Borsodi. Scientific large-scale techniques may be expected to improve nutritive quality, digestibility, sanitary quality and even palatability of our food, and result in much saving of household labor at a comparatively low cost. The employment of expert technicians, appropriate machines and controlled conditions might produce foods of such uniformly high quality when judged by all standards that the average household would find it neither desirable nor possible to compete in much of the actual processing.

In fact, even as it exists to-day, commercial food processing must be given much credit for human welfare. Without such processes as cold storage, pasteurization and evaporation it would perhaps be impossible to insure as much as the present inadequate consumption of milk for the population as a whole. If one food were to be selected for greatest contribution to maintenance of nutrition at as high a level during the depression as has been possible, probably nutritionists would agree that the honor should be given to evaporated milk. The equivalent of fresh milk in all reliable essentials, more certain to be wholesome than average fresh milk, its substitution for the latter in adequate dietaries at minimum cost cuts the grocery bill for an average family $5.00 per month. The re-enforcement of factory-made ice cream, cheese and bread with added milk solids in the form of skim milk powder is a practice which salvages important and often inadequately supplied essentials of human nutrition that should be duplicated in the household as soon as the skim milk powder becomes available in retail units. Modern canning has been rated by some as the greatest of scientific discoveries. Certainly commercial canning of fruits and vegetables gives products which, although they may not rival the fresh in palatability, increase the chance that the average dietary will be properly balanced at all seasons. We have had a few examples of the commercial manufacture of scientifically supplemented foods, for example, iodized salt, irradiated cereals and milk, which have been or may be rapidly adopted by large numbers with improvement in nutritional status. Such a list is by no means complete but is sufficient to demonstrate that, as it exists, the food-manufacturing industry must be given credit for some contributions to nutritional welfare.

Furthermore, commercial food processing has already created much leisure for the housewife—and the average home-keeping woman still works more hours per week than her dinner-pail-carrying husband. Feeding the family requires more time than any other single household function, from 3¼ to 3¾ hours per day in rural districts, somewhat less in towns. The conspicuous waste of leisure by some housewives in socially useless activities should not blind us to the value of time freed from the mechanics of housekeeping by the modern food factory when it is employed in more careful child training, in enriching the social life of the family and in socially

constructive community activities, as well as in creating a more satisfying personal life for the housewife herself.

The removal of much food preparation from the household has undoubtedly made contributions to social welfare and personal satisfaction, though there may be some question as to whether the balancing of benefits against deficits would show a net gain. As organized at present, the critical proponent of consumer welfare might bring two general indictments against the food industries.

In the first place, the manufacturer is primarily unconcerned with the effect of his product on welfare. This may result in nothing more pernicious than the wasteful devotion of a share of our capacity for production to goods which represent no advantage over unelaborated staples, but whenever welfare conflicts with profit-making, the former is generally sacrificed to the extent permitted by our limited protective legislation. As a consequence we have promotion of the unlimited consumption of refined foods like milled cereals and sugar which has caused their consumption to be overstimulated. We have products, such as tomato juice cocktails, which experiments in the writer's laboratory have shown to be unreliable sources of vitamin C, being purchased by the uninformed if not deliberately deceived consumer as worthy rivals of the more certainly antiscorbutic plain tomato juice. We have the patenting of a new bread improver which absorbs 200 per cent. of water and can be used in amounts to produce 15 more loaves of bread per barrel of flour.

On top of numberless such individual perversions of the true function of production, welfare in consumption, consumers are faced with organized pressures against such mild restrictions in their favor as the recently proposed revision of the Federal Food and Drugs Law and the code for canners to require a statement of quality grades on labels. To date such opposition has been effective, probably because we have no strongly developed consumer consciousness in this country.

The second general indictment against the food-manufacturing industries is the charge that they exact excessive remuneration for the services they perform. In the first place, the manufacturer or processor is in a strategic position in regard to price-making. Depending upon the degree of monopoly that he possesses and upon other factors, he more or less decides not only what he will pay the farmer for raw materials but what the consumer shall pay for the product. The depression has enabled him to utilize this power to his great advantage. In spite of efforts of the Agricultural Adjustment Administration to restore what seems to many a more equitable balance between farm prices and prices to consumers, processors and distributors are to-day receiving 62¢ of the consumers' food dollar, as compared with 53¢ in 1929. Pasteurization and distribution make the fluid milk supply an example of an industry so manipulated that since the war ruinously low prices to farmers have been paralleled by relatively slightly changed prices to consumers. In going through the hands of the manufacturer the average food has its money value increased 50 per cent. It is the welfare value of this amount that we are concerned with in this discussion.

In the second place, the food industries have not been free from the inflated salaries and dividends which have characterized large-scale production under our present economic system. The interested reader may turn to the article in the October, 1934, issue of *Fortune* for an account of General Foods which verifies this statement and shows how economies of large-scale machine production and distribution tend to be dissipated before they reach the ultimate consumer.

In addition, the food processor may be accused of levying too heavy a charge upon consumers by supporting costly practices which offer slight or no advantage in consumption. In 1932, a Senate committee found branded goods in packages selling for 60 per cent. more than similar products in bulk. Brands are multiplied until retailer and consumer are bewildered. By a recent estimate we had 10,000 brands of wheat flour, 4,500 of canned corn, 1,000 of canned salmon, etc., and in a single city the housewife must choose from 87 breakfast foods, 46 flours, 93 packaged butters, 101 packaged teas, etc. Not only is it true that in most cases 4 or 5 quality grades are all that are necessary to meet needs, but individual brands have been shown to be of little reliability in identifying uniformity of quality, and consequently do little more than confuse the buyer.

Excessive elaboration is another practice increasing costs to consumers. The modern breakfast food has been aptly described as a pasteboard box of air and advertising wrapped in Cellophane. The consumer may be intrigued by the drama of having his morning cereal "shot from guns," but at present prices this treatment multiplies its cost about 35 times.

Finally, in advertising we have a factor increasing the cost of processed foods out of all proportion to benefits received by the consumer. General Foods, a corporation distributing a group of well-known processsed foods, is said to expend 10 per cent. of its gross sales in advertising. In 1933, this one corporation paid over a million dollars to the publishers of three popular magazines. The trade name of Jell-o was deemed worth $35,000,000 by them in 1933, and their judgment seems to be justified by the fact that this product maintains a position on the shelves of chain stores adjacent to their unadvertised similar product, though the former is held at a 50 per cent. higher price.

In fact, in advertising it has been said that we have a union of the irresponsible industrialists' grasp for profits with a force that not only levies an exorbitant charge upon the consumer, but, as Rorty points out, perverts the integrity of the editor-reader relationship, so essential to a democracy, in all our popular periodicals. Mass advertising makes possible the 5-cent national weeklies and the 10- to 35-cent national monthlies with large circulations and editorial policies dictated by the advertisers. The writer knows of not one that defended the lately proposed food and drugs legislation which asked for simple truth in advertising.

Whether or not the natural scientist denies responsibility for the use made of his discoveries, as a member of society he can not escape the consequences. Controlled reformation of our economic order to insure the more complete utilization of the results of science to improve the welfare of all is admittedly a very complex problem. Perhaps he is unequipped to make important contributions to its solution. However, both as a member of a vocational group claiming the socially responsible rank of a profession and as a citizen possessed of a minimum degree of civic consciousness, he must be critically interested in suggestions for such adjustments. Thus we proceed to proposed methods for improving the consumers' position in relation to the processing of food.

Three lines of action have their proponents. A first is the continuation and extension of the present method of enacting legislative regulation of industrial practices to protect the consumer. The principal weakness of this policy is the lag between the development of the need for restriction and the development of sufficient consumer consciousness and organization to effect the pressure required to outweigh the efficiently organized opposition. The struggle for consumer purchasing power, whatever the

cost to consumer welfare, is likely to be more aggressively pursued than any campaign for his protection. Years of agitation during which unremediable damage to health may result are often required, as the great need and struggle for the food and drugs legislation of 1906 as well as for its revision in 1934 bear witness.

A contemporary trend in food processing which may have peculiarly pernicious results because of the indirection of its effects and the lag in their recognition and control is that of improving the palatability of processed foods. It is highly significant for the consumer that one industrial chemical company announces a new house organ on taste and odor control. The editor of the trade journal, *Food Industries,* writes: "One of the next constructive steps in the production of manufactured foods should be that of conferring upon factory-prepared foods an elusive something that can not be attained in the home. . . . No surer way exists for a manufacturer to maintain his prestige with the housewife than to produce better tasting foods than can be prepared in the kitchen at home." This may seem a laudable exhortation to the reader, but the nutritionist is reminded of the inadequacy of palatability as a guide to food choices and of the propensity of the human race to follow it. In the past the failure of many commercial products to compete with those prepared in the household in flavor has been some measure of protection for the consumer, retarding the adoption of foods which have had their nutritional imbalance increased during processing. When the chemist is employed to create appetite appeal, most of us will be led where he wishes, regardless of truths about composition on labels or in advertising. Possibilities of such attempts are indicated by the reported discovery of a compound which imparts a full butter flavor to a butterless substance, though present only as one part in 50 millions and another which gives a meat flavor when present in minute quantities.

A second method suggested to protect the consumer from practices of the food industries which are unwholesome or uneconomic is the boycott. This may be carried merely to the extent of buying only staple, relatively unprocessed materials in so far as possible and preparing them in the household, as advocated by Consumers' Research, or to the extent of establishing almost complete independence of the food industries by retreat to the haven of a subsistence farm, as urged by the Borsodis. One of the latter, in two books entitled "This Ugly Civilization" and "Flight from the City," describes their experiences as city intellectuals transplanting themselves to a few rural acres where they produce and process most of their family food supply. They maintain that the average woman can still earn more in her own kitchen than in factory, store or office. A certain nostalgia for the simple life that this is seen to represent awakens a sympathetic if not a sentimental response in many of us. But a return to this way of living, doubtfully idyllic even when accompanied by cheaper power and modern labor-saving gadgets, will probably be only an emergency solution for a comparative few. It is unlikely that the skills and hours of household labor required for such an existence would remain either interesting or endurable to those who have lived in emancipation from them or to those whose social and creative aspirations are not satisfied by such a revival of an agricultural economy.

A third response to the problems which science and technology have brought upon man as consumer is based upon a belief that these agents for creating the leisure essential for the good life may be controlled in the interests of all. As Mumford points out in a magnificent analysis of "Technics and Civilization" . . . "while technics undoubtedly owes

an honest debt to capitalism . . . it was nevertheless unfortunate that the machine was conditioned, at the outset, by the foreign institutions and took on characteristics that have nothing essentially to do with the technical processes or forms of work. Capitalism utilized the machine not to further social welfare, but to increase private profit." He proceeds to reason that the full benefits of the machine can come only with a basic reorganization of our economic system, a rationalization of production directing it toward consumer needs rather than producer profits. Then the economies of large-scale machine production, the quality that can be secured under controlled conditions by expert technicians, and the increased leisure may be more fairly shared by all.

The execution of such a reorientation of production will be no simple or easy task. The relative merits of democratic consumer cooperation, socialistic government ownership, fascistic and proletarian dictatorship are widely debated in all the contemporary world. In the secure fastness of his laboratory the natural scientist would perhaps seek peaceful refuge from the conflict without but complete immunity from the pains of our society's adolescence or senility, whichever one deems them, is not likely to be granted.

MAKING MALARIA WORK FOR THE DOCTOR

By Dr. NORMAN TOBIAS

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We have often heard of such trite expressions as "returning good for evil" and it takes "poison to kill poison." Such loose ideas may truly be applied to a disease that is feared throughout the world and numbers hundreds of thousands of victims yearly, namely, malaria. And yet through an accidental discovery this disease has been found to be of inestimable value, as an instrument of good, as a medicinal measure in curing another condition that has caused much more suffering and harm, that human scourge, syphilis, of which there are more than 6,000,000 cases in men, women and children in the United States to-day. Now syphilis is a many-faced disease with symptoms so mild in the beginning that many victims fail to notice them. In an unfortunate 10 per cent., the nervous system often becomes involved, causing mental disturbances or paresis if the brain is affected, locomotor ataxia, if the spinal cord is attacked, or partial and complete blindness if the optic nerves are implicated.

The use of fever or, to be more explicit, inoculating one febrile disease to cure another is not a new discovery. Garrison in his "History of Medicine" points out that the beneficial effects of malarial infection in epilepsy were known to Hippocrates. The ancient Greeks and Romans applied the principle of heat treatment in certain disorders by advocating the use of hot baths and mineral springs for many diseases, a measure which persists to this day. Physicians often note an improvement in many chronic disorders complicated by such febrile diseases as erysipelas. Down through the ages medical men have observed the improvement in chronic skin diseases following in the wake of superimposed attacks of scarlet fever or other contagious diseases.

These older methods of inoculating one disease to cure another are now being used again throughout the world. The germ of erysipelas is sometimes used to combat a form of cancer called sarcoma. Dead typhoid organisms are often in-

jected into the blood to relieve arthritis or other chronic diseases. Inoculating a syphilitic with the germs of relapsing or rat-bite fever as a means of provoking fever in overcoming serious complications in the nervous system has been used in certain countries. All of which brings up the question: why graft one disease upon another serious disease and expect good to come of it? It is all a matter of recognizing fever or internal heat as a factor of good, as a curative agent.

Most folks regard fever with alarm and take active measures to reduce or "break it" as soon as possible. In most cases this is perfectly proper, because fever is uncomfortable and often associated with other symptoms. But still the fact remains that fever is a defensive mechanism on the part of the body to overcome destructive and foreign invaders and signifies that a battle royal is on to determine who shall win, the patient or the disease. The process of fever production sets up certain blood and tissue reactions which usually destroy the harmful infection. Serious infections with little or no fever are usually fatal, and medical men have come to realize that fever in itself is a good prognostic sign. These facts were recognized as far back as 1870, when Weigert proposed his law stating that "local injury or necrosis usually sets up a reparatory process in excess of requirements." Or in other words, as Pflüger stated in 1877, "injury is the incentive to the removal of injury."

Unfortunately, when Paul Ehrlich discovered salvarsan in 1910 he believed that he had at last found a sure cure for syphilis, but subsequent observation has brought out the fact that where the disease has been present for a long time and has invaded such delicate tissues as the liver, heart and brain very little hope of cure can be expected from drugs alone. Chemical treatment can improve the disease to a certain level, but beyond that the response is slight. So fever treatment has come into the field of therapeutics as a remarkable aid in chronic resistant syphilis, whether acquired or congenital, and more specially and successfully in paresis.

It remained for a kindly, humane, eccentric professor in Vienna to assemble and correlate his observations on the effect of fever in mental diseases and to apply it in his daily work. That man was Julius Wagner-Jauregg. This great observer, who recently celebrated his 76th birthday, has been a professor at the University of Vienna for 44 years. He began his studies as a student in that same university under the great internist, Bamberger, the well-known pathologist, Stricker, and the neurologist, Leidesdorf. The latter interested him in mental diseases early in his studies and developed in him a sympathy and practical interest for the unfortunate insane. At the time that Wagner-Jauregg made his observations on the effects of fever treatment in mental diseases, psychiatric treatment was highly theoretical and most of his colleagues were therapeutic nihilists or treatment scoffers. As this kindly professor with bushy eyebrows and large mustache made his daily rounds in the hospital, he took careful notice of each and every one of his mental patients. He was especially astounded at the remarkable amount of improvement in one of his paretic patients who developed erysipelas. This gave him great food for thought, and after assembling data acquired through reading the literature, and observations of many doctors before his time, he first published his theory of fever treatment in 1887. However, his older colleagues jeered at him and criticized his work severely. In 1891 when he began to use tuberculin as a means of producing fever, not only did the faculty rise up in arms against him, but the entire Viennese press printed editorials caustically

criticizing his work and holding him to be a potential murderer. However, Wagner-Jauregg was tolerant of his critics and quietly continued to study the problem. In his study of endemic cretinism in Styria he developed powers of organization with mass treatment which later fitted him to inoculate hundreds of patients with malaria and tabulate the results.

With the onset of the great war his work was more or less forgotten until 1917 when he resumed his experiments. At this time the faculty consisted of many new, young and open-minded members who quickly took recognition of his work. He began by treating paretic soldiers with malarial inoculations which he found to produce the best results. His work in this field has been so successful that it is now being used throughout the world. His recognition was climaxed with the bestowal of the Nobel Prize in 1927.

When we speak of malaria we mean a particular type called benign tertian caused by the plasmodium vivax parasite. This type of malaria is practically never fatal and can be readily cured by administering quinine. The Middle West and Southern states are practically never free of it and sporadic cases occur in every state of the Union. It is naturally transmitted by the bite of the infected Anopheles mosquito. In contrast to this mild form of malaria we must recognize the more serious or malignant form which is never used in the treatment of disease, namely, aestivo-autumnal malaria.

Since infected mosquitoes are not always available and for practical purposes are not ideal nor safe sources of malaria, physicians utilize the blood of patients already infected with the disease. The syphilitic recipient is injected under the skin with blood from the malarial donor and the physician waits for the "take" to occur. This usually requires about 10 days, after which the paroxysms of fever and chills followed by sweating occur with more or less regularity. In the 24 to 48 hour free intervals the patient is usually comfortable, but during the attacks of fever he appears quite ill and has a temperature of 104° or higher. The patient is permitted to have 8 or more paroxysms and then the malaria infection is overcome rapidly by administering quinine. The peak of success from this type of treatment occurs in about 12 months with gain in weight and marked freedom from mental and physical ailments.

Post-mortem studies have shown how malaria acts on the nervous system. It sets up a powerful stimulation in an extensive group of protective tissues called the reticulo-endothelial system which is found in the spleen, bone-marrow, liver, brain and lining of the smaller blood vessels. This stimulation produced by the malarial germs causes an exodus of cannibalistic cells called histiocytes or wandering cells, which rush into the capillaries of the brain and other organs and devour the hidden, harmful nests and deposits of the syphilitic germ which have interfered with the normal blood supply and breathing of the brain and nerve cells. All this through the medium of the fever which sets the wheels in motion for the malarial organisms to do the work. And so we can be thankful to Professor Wagner-Jauregg for giving us malarial therapy in treating diseases which long have been regarded as hopeless and incurable.

SCIENCE SERVICE RADIO TALKS

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THE MINOR PLANETS: STRAY SHEEP OF THE SOLAR SYSTEM

By Dr. A. O. LEUSCHNER

PROFESSOR OF ASTRONOMY, UNIVERSITY OF CALIFORNIA

THE solar system, of which the earth on which we live is a part, consists of a host of celestial bodies whose motion is controlled primarily by the gravitational attraction of the sun. This attraction causes them to move around the sun in more or less elongated ellipses, known as orbits, with various periods of revolution, ranging from 88 days for the planet Mercury, nearest to the sun to thousands of years for some comets.

Best known among the members of the solar system are the major planets Mercury, Venus, Earth, Mars, Jupiter, Saturn, Uranus and Neptune, in the order of their distances from the sun. They have relatively large mass, or weight, and size. The heaviest of them, Jupiter, has a weight approximately one one thousandth that of the sun. There is also Pluto, discovered in 1930, far beyond the orbit of Neptune. Pluto is still classed as a major planet, because of the expectation of astronomers that another such planet existed beyond Neptune, but its weight and size have been disappointing, being probably less than those of the moon. The moon's weight is about one eightieth that of the earth. The earth's weight in turn is about one three hundred thousandth part of that of the sun. Other major planets besides the earth have moons revolving about them, but we are not concerned with them to-day.

The distances of the major planets from the sun range themselves in a certain sequence, known as Bode's law. This law is entirely empirical, but it led astronomers to suspect the existence of another major planet between Mars and Jupiter, at a distance from the sun of approximately 2.8 astronomical units, the astronomical unit being the distance of the earth from the sun. Organized search by astronomers failed to reveal such a body, but on January 1, 1801, the Sicilian astronomer Piazzi accidentally noticed a strange moving body while looking for a certain star. He named it Ceres, and it was hailed as the missing planet predicted by Bode. Its distance fits Bode's law, but its size and weight, like those of the recently discovered Pluto, were a source of great disappointment. The diameter is only about 500 miles, less than one seventh the diameter of Mercury, the smallest known major planet. Still more puzzling was the discovery of three more such objects by 1807. In distinction to the major planets, these small bodies became known as minor planets. Ceres, the first discovered, went astray, and soon was lost from sight. Fortunately, months later it was located again among the stars, as a result of calculations under the Newtonian law of gravitation.

This straying from their paths of the minor planets presents perplexing problems to the astronomer. Straying sheep are rounded up by the shepherd's watchful dogs, and are brought back to the path which the flock is to follow. Minor planets, however, have to be followed individually by arduous computations if they are to be sighted again. Their breaking away from the elliptical orbit,

however, is not capricious, as in the case of sheep, but is due to the universality of Newton's law of gravitation, which subjects the minor planets not only to the attraction of the sun, but also to that of the major planets of the solar system. The fact that we know why the minor planets stray from the paths prescribed for them by the sun offers little comfort to the astronomer, considering the enormous difficulties involved in calculating the action of the major planets, known as perturbations.

The discovery, up to 1807, of four minor planets between Mars and Jupiter stimulated search for others. By 1890 more than 300 of them were known. Up to that time the discovery of minor planets was an arduous task. In the telescope a minor planet looks like a faint star. To identify it as a planet it is necessary to observe that it moves relatively to neighboring stars. That means watching the stars visible in the telescope, at least for several nights in succession. This can only be done with the aid of accurate maps of the fainter stars. Such maps, therefore, had to be made with great labor.

The planet's orbit around the sun generally can be approximately calculated if its position has been measured accurately at three different times. The accurate positions are secured by measuring parallel and perpendicular to the equator the planet's angular distances from the position of a star. The accuracy of the planet's measured position thus depends on the accuracy of the star's position.

Beginning with the year 1890 the flock of minor planets began to multiply enormously with the use of photography in searching for them. A plate exposed in the focus of a photographic telescope which follows the daily motion of the stars by clockwork and other guidance, reveals the stars as points on the plates, while the moving minor planets produce small trails. The position of the minor planet relative to the stars may be accurately measured. Further exposures are made in the days and months that follow to secure the positions necessary for the calculation of an orbit.

Originally astronomers followed and accurately measured every new minor planet. This requires the calculation of its approximate elliptic path. Otherwise it will go astray and that is what is happening now with the vast majority of new minor planets. At the time of discovery they are near the earth, with the sun in the opposite direction. As a planet moves away from this favorable position it becomes invisible. After at least a year or more, it again revolves into the favorable position of opposition. To find it then, the perturbations of the major planets must be taken into account by complicated mathematical methods. It is not a question of keeping minor planets rounded up like a flock of sheep, but every planet goes its own way and has to have an individual shepherd who subjects its wanderings to calculations.

The largest number of minor planets discovered in any year was in 1931 when 391 were found. Since 1910 over 3,000 have been discovered. Of this large number only one in five has an orbit computed with adequate accuracy to warrant a prediction of future motion with confidence. Such planets receive a current number. So far, 1,301 planets have received numbers out of more than 5,000 discovered since the beginning of the last century. For some 40 of these, the predictions at best must be uncertain. For an additional 85 search so far has been of no avail or if found, they had strayed far from the predicted path. Another 306 of the 1,301 have either not been seen since 1930 or they have not been observed on sufficient returns for reliable predictions. Not all minor planets which show as trails on plates are listed, but sample counts of selected regions have been made from available plates which reveal that there are about 50,000 minor planets within reach of the largest telescopes.

The early observations may be represented by quite a range of preliminary orbits, and only later observations can reveal how close the adopted preliminary orbit is to the real one. When a preliminary orbit is known, so-called special perturbations due to major planets may be calculated step for step with their known weights or masses. This is repeated until an orbit results which with these special perturbations enables the astronomer to predict the position of the minor planet. But it has not been possible to keep track in this manner of all the numbered minor planets. There is another method known as general perturbations which gives the total effect of the action of the major planets at any future date. This method is used increasingly and has so far been applied to 223 planets. It is particularly effective if planets of similar orbits are taken in groups, as, for example, all the planets which move around the sun twice as fast as Jupiter, more or less. No planet moving less than 700 seconds a day can be followed without consideration of the perturbations. If its motion is between 700 and 850 seconds and the ellipse is only moderately elongated, or if the motion is more than 850 seconds and the ellipse is highly elongated, there is also no possibility of adequate prediction without consideration of the perturbations. The process of general perturbations enables the discoverer to pick up a minor planet after decades without the intervening observations or calculations necessary with special perturbations.

Gradually the great significance of the study of the motions of the minor planets is becoming more and more apparent. May I touch merely on a few of the problems, in which they are being recognized as having an important part. Precise star positions and maps are required for some of the most important studies now under way, such as the structure of the universe. While precise star positions originally served for the accurate determination of planet positions, it is now recognized that when the motion of the planet is precisely known, the positions of the stars which it passes in its course can be improved by reference to its orbit.

Motion involves intervals of time. Our daily time is measured by the rotation of the earth. Irregularities of the rotation of the earth would cause apparent straying of a minor planet. In turn, any existing irregularity in the rotation of the earth may come out of the study of planetary straying.

Many problems depend upon an accurate knowledge of the distance of the earth from the sun, our astronomical unit. Orbits in the solar system are calculated with this unit as a scale. If the minor planet in its elongated orbit should come sufficiently close to the earth so that its distance can be measured in miles by triangulation, the exact length of the astronomical unit may be determined. There are several such planets, one of which cuts into the orbit of Venus and under the most favorable circumstances can come as close as six and a half million miles to the earth, as compared to the sun's distance of ninety-two and a half million miles. Unfortunately, the few highly important minor planets of this kind, except one, have gone astray. The exception, known as "Eros," has been used in two international astronomical campaigns for the determination of the sun's distance.

Straying of minor planets, as has been noted, is caused by the major planets. *Vice versa,* a close approach of a minor planet to a major planet enables us to determine the latter's mass or weight. Minor planets have many points of resemblance to comets, another group of bodies of the solar system. Some of these travel in highly elongated orbits with periods running into thousands of years. Comets and minor planets have sprung either one from the other or from a common source, yet to be ascertained.

Minor planets are observed from a

moving earth, and the calculation of their orbits requires an accurate knowledge of the earth's orbit around the sun. With all other causes of straying eliminated, the motion of the minor planets gives opportunity of perfecting our knowledge of the orbit of the earth.

These are but a few illustrations of the parts minor planets play in the scheme of astronomical science, but how can we account for their existence and the variety of orbits in which they move? Evidently much light might be thrown on this question if by perturbations traced backwards into the distant past we could lead them back to where they came from. Such investigations have been made and it has been found that at certain epochs in the past families of planets started from the same point with reference to the sun. Each family may thus be the remnants of a disintegrated larger body. Whether the parent bodies, if traced still further back, again be found to be parts of a still larger original body is at present a matter of speculation. Such parent bodies may have given rise simultaneously to comets and minor planets.

Thus minor planets and comets must be accounted for in any theory of evolution of the solar system. They form as important a part in this problem as the study of the distances and motions of the nebulae in the problem of the nature of the universe.

THE MOSAIC OF NATURE

By Dr. GEORGE J. PEIRCE

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If a space of air and soil were absolutely cleared of every living thing, by war or fire or volcanic eruption, what would follow? What has followed? By degrees, and by invasion or immigration or spreading from the surrounding air and soil, the space has become occupied and fully occupied, as before. There has been reestablished that balance, that mosaic, of nature which exists in every new country and in every old country sufficiently old and undisturbed. In every country less new, and in every area only recently devastated, the mosaic is still in the making. Not only is the mosaic still in the making, but there is such a shuffling among the possible components that some of the pieces may be broken and the pattern may be greatly changed.

This reconstruction is not like that which follows an evening's work upon a picture puzzle, the restoration of pieces of various shapes to the spots from which they were sawed and taken. Nor does it follow a design preconceived in the mind of the artist and prefigured to the eyes of the artisan. Instead, the reconstruction results from repeated rearrangements among themselves of the moving, living, struggling pieces of the picture.

We may call some of the pieces plants, others animals; but in doing so we must include ourselves also, for man is a part of nature. To be sure, it is quite contrary to man's absurd estimate of himself to think of him otherwise than as "the lord of creation"; but while man sometimes thinks of himself as "controlling the forces of nature," it requires only a moderate earthquake or even the procession of the seasons to show him that he is powerless even to delay the movements of the earth, either the wrinkling of its crust or its turning upon its axis.

He has adjusted gravity to his own benefit, and makes it support his skyscrapers and furnish him with hydroelectric power; but the birds do better,

and even the trees—furnished with running water throughout!—anticipated his high buildings. He has built houses for protection and defense; but fur and claws accomplish both. He has devised societies; but no more perfect or monotonous democracy than the forest, no more complete subjection of labor to capital than his own dependence upon water and the products of the soil. He must struggle for space and for subsistence with the other living—and with the lifeless—components of the mosaic of nature. And ultimately he succumbs to a staphylococcus or a deficient vitamin or an unbalanced hormone, and so keeps his place in that continuing procession of living beings stretching from the beginning of time until its end.

We must recognize, too, that while all these living pieces of the mosaic perform constructive rôles in the making of the picture, some are constructive in the sense of manufacturing and building, while many others are destructive, living upon and destroying the products of the first. The sheep is dependent upon the grass which makes sugar, but man depends upon the sheep for food and warmth. Both man and sheep return to the air and soil the carbon dioxide gas, the mineral matters and the water of which their bodies were built.

An impressive part of the mosaic of nature is this maintenance of soil and air fertility for generation after generation. As long as there is no decline in the supply of energy from the sun for doing the work of the world the useful composition of air and soil will be maintained. For in the hours of daylight, while carbon dioxide gas is being absorbed, abstracted from the air by green leaves and combined with water to make food in the form of sugar, the oxygen content of the air is maintained by the oxygen liberated in food manufacture. As constantly, each living thing, by its respiration, is returning carbon dioxide and water to air and soil. By these two processes the mosaic of nature is kept intact. The pressure and the composition of the air remain the same in any given area for century after century.

In the mosaic of nature there is generally food enough for all. There may be starvation, death, locally, as the result of war or other destructive calamity; but in the scheme of nature production and consumption are equal. There is no accumulation, nor is there any deficit. Those plants and animals which are short-lived make and store enough food to satisfy their own requirements, to enable them to reproduce, and to provide their offspring in seed and egg with sufficient food to carry them through that period in which they are too small or too weak to shift for themselves. Longer-lived plants and animals accumulate in their own bodies such stores of food as will carry them over the periods of non-production, over the winter, through the dry season; and some of the cleverer animals have acquired the arts of harvesting and storing so that, although they grow thin, they do not necessarily die.

It is true that this general balance of production and consumption, of supply and demand, while wonderfully perfect throughout nature as a whole, is not always and everywhere attained. Individuals, communities or larger groups of organisms may suffer or even perish. Nevertheless, the oceans maintain their populations of fishes, seaweeds and plankton unchanged year after year, unless disturbed by some intrusion of creature or of force. The land each season produces its blanket of grass and herbs or adds height to its forest cover and so maintains its animal population year after year. Here and there invasion, which may exhibit itself as war or as epidemic, according to the size of the invader, may destroy a part or the whole of the living population. Man may fell or fire the forest, mosquitoes may distribute malaria, floods may devastate, drought and wind may even remove the soil. Yet all these departures from the

general scheme reveal themselves because they are exceptions. Generally there is food enough in nature for all.

Man possesses qualities which distinguish, sometimes mislead and often threaten to destroy him. He has more inventive power than any other living thing. Some of his inventions come dangerously near being fatal to him. He is called a social animal. His invention of the city illustrates both. For a city is of all things the least able to support itself. If it grows too fast or too large it will outrun the means of subsistence and die of hunger in cold and filth. Man's immodest effort to depart from the pattern of nature has resulted in certain immense advantages to himself individually; but it has exposed him at the same time to grave personal dangers, and more than once it has involved threatened if not actual racial destruction.

Not only by interfering with the mosaic of nature through concentration of individuals into limited areas, but in numberless other ways also man has distinguished himself as both ignorant and bold. Without study and knowledge of the world he lives in he determines to do what desire dictates. He fells the forests, clears the land, sows an imported grain, combats all previous occupants of the area, regardless of the doctrine of prior rights, endures the caprices of the weather, harvests the survivors of the struggle for existence between the old families and the newcomers, eats and sows these survivors and conceives himself to be prosperous if he has a salable surplus.

But if his breaking the native sod, clearing the land and sowing to grain be followed by prolonged and intense drought, the crops will fail, there will be no cover and no binder to the soil, the wind will pick it up, carry it away and finally deposit it in those places where it is least desired. On the marginal lands of the Dakotas and of the western dry belt the would-be farmer smashed the mosaic of nature. He has suffered variously for years, producing crops of political, social and economic ideas repugnant to those less bold or less original persons who remained where water, soil and climate are less threatening. Finally, the continued threat has been carried out, good soil has been lifted and whirled away, the former owner impoverished, the recipient embarrassed. A return of the soil to nature is impossible. The mosaic is not only broken, it is dissipated.

Similar danger threatens man's use of sand dune areas. If he increase whatever holds the sand together, be it plant cover, suitable buildings or binding oil or asphalt, then is his procedure safe; but let him cut the natural coat, sever the natural binders, and the sand will begin to move, irresistibly, in the direction of the prevailing wind, burying farms and settlements or shifting channels, but in the end establishing a new and not always welcome pattern of the mosaic.

Erosion control is more than a charitable impulse, a vote-getting device or a euphemism for a dole. It is a conscious, intelligent effort to repair, as rapidly as possible, what has been damaged by road cutting, by down-hill instead of horizontal plowing, by the destruction of the forest cover by fumes and by other interferences with the established pattern of nature. It checks the run-off after rain and melting snow; it prevents the scouring and scoring of hillsides; it reduces the risk of freshets; it lightens the load of silt carried by the streams. It is one of the finest examples of man's undoing by cooperation the harm which he has done individually in unintelligent self-interest. It is a partial fulfilment of the promise which the biologist sees of increased comfort, improved health and greater happiness for all in such study and understanding as will lead man to be a harmonious and not a rebellious part of the mosaic of nature.

THE ROMANCE OF MODERN EXPLORATION

By ANSEL FRANKLIN HALL

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THE story of exploration in the desert Southwest offers opportunity for introducing a bit of the thrill that goes to men who are privileged to push into new country. It may surprise you to know that this land of ours still offers opportunities for real exploration. There are still vast areas that are as yet practically unknown to white men—areas that challenge the interest of the scientist and the man who would seek the last frontier of the old West. Off in the dim blue distance far to the northeast of the Grand Canyon of the Colorado lies a vast, colorful, picturesque country—the home of the Navajos and of prehistoric tribes that developed a civilization that disappeared from the face of the earth long before the new world was discovered by the white man.

But this country is not all desert. The sacred mountain of the Navajos lifts its huge bulk so far into the blue that its 10,000-foot summit is clothed with a dense forest of pine, spruce and fir such as one would expect to find 2,000 miles farther north—but certainly not in the desert Southwest. Surrounding its flanks is the great Rainbow Plateau, which is cut by deep canyons in which prehistoric cliff dwellings nestle beneath the overhanging ledges. Farther toward the east, high straight-sided mesas are a common feature of the landscape, and desert erosion has caused the weird and unique features of the Monument Valley. Here are to be found natural bridges in various stages of evolution, the most perfect of all resembling a huge rainbow which lifts its arch higher than the dome of the national capital building in Washington.

Into this weird country went a band of explorers in the summer of 1933 and again in 1934—engineers, geologists, biologists and other scientists whose duty it would be to map the area and to penetrate as far as possible into the interior in a preliminary reconnaissance that would determine the plans for future field seasons of more intensive exploration and scientific investigations. These men were all volunteers—men who during the remainder of the year were students and instructors in the universities and colleges of California and the East.

When the auto caravans left California and New York City early in June on the trek to the Southwest, the spirit of adventure ran high. The first several hundred miles of the journey were clear sailing along wonderful highways, but at Flagstaff, Arizona, the party turned north into the desert. Roads became worse and worse. Deep sand and chuck holes resulted in two broken axles. The last hundred miles were scarcely more than a track across the desert. Finally the supply base was reached—the little outpost called Kayenta at the northern end of the Navajo Indian Reservation. From here on the party must make its own roads and trails. Here the expedition was divided into smaller field parties, each of which would push into the unknown afoot and with pack train or would endeavor to find a way across the desert in their sturdy station wagons. Before the field parties set out, however, all members of the expedition helped to construct a landing field, for the expedition's reconnaissance plane was due to arrive in two or three days; the nearest airport was 175 miles away toward the south. Meanwhile the pack train was organized and the archeologists and biologists started northward into the canyons of the Tsegi in an attempt to cross Skeleton Mesa to meet at an ap-

pointed rendezvous on the flanks of Navajo Mountain a month later.

The following days were filled with real adventure for the archeologists. Frequently large prehistoric ruins were to be found nestled in recesses under the overhanging cliffs. Some of these ruins were accurately dated by means of borings taken from the timbers which had been used in their construction. In other places the only indications of primitive inhabitants were the fragments of broken pottery left here almost one thousand years ago. The birds, mammals, reptiles and plants of the region, as they exist to-day, in themselves present an interesting study—but all have a bearing upon the life of the ancient people. The biologists made a definite effort to correlate these field studies with the archeological discoveries.

Meanwhile the engineers were at work with plane tables and transits running stadia traverse and triangulation which would enable them to produce a base map of the area. We had expected when we entered the country that there would be some 700 square miles to study, but the region proved much bigger than we had anticipated, and we soon discovered that we had on our hands the problem of exploring and mapping some 3,000 square miles. A preliminary map was made, and this year we expect to return to concentrate upon the making of an accurate contour map of the areas where intensive field work will be in progress.

Bounding this region at the north are the deep canyons of the San Juan and Colorado Rivers. Entirely inaccessible by trail—the only means of penetrating these chasms seemed to be by boat. It would be a hazardous venture, attempting to make the passage through these 200 miles of canyons—but there was no lack of volunteers to man the seven small boats which were specially constructed for the purpose. There was anxiety in camp on the day that the small fleet was launched on the muddy waters of the San Juan. Would they successfully run the rapids and come through unscathed? After three weeks—weeks of anxiety for other members of the party—a truck was sent to Lee's Ferry at the lower end of Glen Canyon to bring back the pioneers and whatever boats might have survived the ordeal. But the fleet arrived without mishap. The members of the party told exciting stories of cliff ruins and Indian pictographs, and of the unique fauna and flora that must certainly be studied during future field seasons. The passage through the canyons had been demonstrated to be practicable, and we expect this year to send another group of explorers and scientists into the canyons to make detailed studies.

Our geologists found many intriguing problems in this almost unknown region. Several of them engaged in the fascinating game of hunting fossil dinosaur footprints. Many were discovered, some of them large enough to have been made by the giants of the ancient world, others as small as the footprints of *modern* lizards. Plaster casts were made of more than 200 impressions so that actual facsimiles can be made for the purpose of further study.

But with the exception of the Chinle, the formations of the Navajo country are almost barren of actual fossils. Keen-eyed geologists had been looking for fossils in the Navajo Sandstone for half a century and never a vestige had they found of anything that might tell of life on the earth when these thousands of square miles of red sand had been laid down. Would *our* geologists be fortunate enough to find such a treasure? Surely in all those thousands of square miles there might be—but no, week after week went by, and boots, clothing and patience were worn and tattered from scrambling over sandstone and climbing down into seemingly inaccessible canyons. No, the search was a failure. But hope dies hard. On the last day of the

season, when the members of the main party were assembling their belongings in the base camp for the morrow's start on the thousand-mile trek back to the Pacific Coast, four geologists said, "Let's make just one more final search in the rocks up in the Tsegi Canyon." They did. Disappointment again. Knocking off work at sundown they started the long wearisome 10-mile hike back to camp. The red sky turned to purple. It was getting hard to see. Suddenly one of the men said, "What's this?" Just a light spot on a red rock that almost any one, even a geologist, would have passed by. But it looked odd. A tap of a geological hammer and there, sure enough, lay a white bone embedded in the rock. A fossil *at last* in the Navajo Sandstone! A discovery of such importance would normally have been left in place, but this was disintegrating surface rock, so there was nothing to do but carefully gather the fragments.

Late that night the party, jaded but triumphant, burst into the campfire circle with the startling news. And it proved to be even more startling than they suspected, for when these precious fossils were sent to the University of California for identification and study they proved to be the remains of an entirely new type of small bird-like dinosaur. And most important of all, this find represented the very first fossil of any kind ever found in the Mesozoic rocks of the Southwest.

1934—second assault upon the northern Navajo country—discovery of a very ancient Basket Maker burial cave, which yielded rich finds—and the unsolved mystery of seventeen headless mummies; then there were mammals, birds, reptiles, insects, many of them never before reported from Arizona, and some of them entirely new to science; also an unexpected geological find in a small cave that produced more than 100,000 fragments of Pleistocene bones—an entirely new fauna for this region; time prevents our listing even the major thrilling discoveries which resulted from that season's intensive field work.

But the job of exploring and studying this fascinating area of desert, mesa and canyon is far from completed. Several seasons of field work will be required before the major problems are solved. Some 2,000 square miles still remain beyond our present horizons. And so about the middle of June another party of scientists and explorers left New York, under the leadership of Dr. Charles Del Norte Winning, of New York University, while at the same time the western caravan started from California. What does the season of 1935 hold in store for these adventurers? Nobody knows! That's what makes exploration such a thrilling fame!

INTERNAL FRICTION IN SOLIDS

By H. WALTHER
PHYSICAL RESEARCH DEPARTMENT, BELL TELEPHONE LABORATORIES

When a bar of carefully annealed aluminum is held in the middle and struck at one end, the sound emitted by it may be heard for more than a minute. If the same is done to a bar of lead, the absence of any musical note is usually dismissed with the remark that the bar is "dead." Now, of course, the aluminum bar is just as dead as the lead bar, if both are regarded as inanimate objects; and the lead bar is just as much alive as the aluminum if both bars are considered in terms of their constituent atoms or electric charges.

Then why do we hear the ring of the aluminum bar and not the lead? The answer lies in their difference in behavior toward mechanical vibration. In the aluminum bar the longitudinal vibration produced by the blow is relatively vigorous and is sustained because internal resistance to motion of elongation and contraction is small. The energy imparted to it is gradually radiated into the air, and some of it reaches our ear. In lead, on the other hand, internal resistance to strain motion is enormously large as compared to aluminum. The result is that the vibration is in the first place only great enough to be barely audible, and furthermore dies out so fast that within one tenth of a second the displacement at the ends of the bar is reduced to about one millionth of its original value. In this case practically all the energy is *dissipated* within the bar and goes to raise the temperature of the material. The amount of heat generated, of course, is very small indeed, due to the extremely small amplitudes of motion involved. For instance, in a lead bar ½" in diameter and 30" long, a light blow at the end produces an amplitude of motion of about 10^{-7} cm. The corresponding rise in temperature would be roughly 10^{-14} degree centigrade per second, a rate of heating which, if sustained under perfect insulation, would require three million years to raise the temperature of the bar by one degree!

As low as this rate of dissipation of energy in lead seems, it actually is unusually high among solids. To distinguish between various solids in this respect there is used what is known as the *dissipation constant* of the material. This quantity is based on vibrational measurements of a particular sample of the material and is defined as the ratio of mechanical reactance to mechanical resistance of the sample. Mechanical resistance and reactance are terms quite analogous to the more familiarly known corresponding electrical terms. Thus in a bar vibrating longitudinally, the mechanical resistance measured at one end is simply that quantity which when multiplied by the square of the velocity of motion of that end gives the power dissipated in the bar. Just as an electrical system, a mechanical system involves two reactances. One is the mass reactance, which is the product of effective vibrating mass by 2π times the frequency; and the other is the elastic reactance, which is that quantity which when multiplied by 2π times the frequency gives the force per unit displacement. The two reactances are numerically equal when the bar is vibrating at one of its natural frequencies, and so it is immaterial which one is used in the calculation of the above ratio. The elastic reactance, also called stiffness reactance, is the more convenient one to use. In a bar vibrating longitudinally, the elastic reactance is proportional to Young's

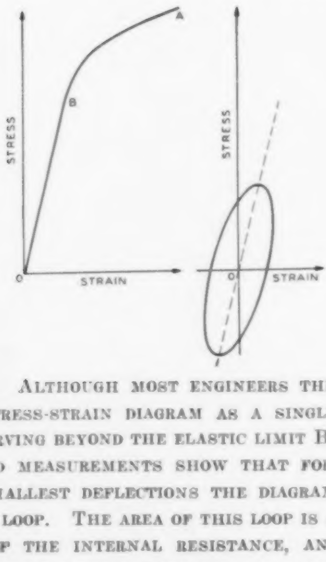


FIG. 1. ALTHOUGH MOST ENGINEERS THINK OF THE STRESS-STRAIN DIAGRAM AS A SINGLE LINE OA CURVING BEYOND THE ELASTIC LIMIT B, MORE REFINED MEASUREMENTS SHOW THAT FOR EVEN THE SMALLEST DEFLECTIONS THE DIAGRAM IS A CLOSED LOOP. THE AREA OF THIS LOOP IS A MEASURE OF THE INTERNAL RESISTANCE, AND THE SLOPE OF ITS SIDES AT ANY POINT IS A MEASURE OF THE ELASTIC REACTANCE.

modulus E, and the resistance is proportional (by the same factor of proportionality) to $\omega\ \mu$, where $\omega = 2\pi f$ and μ is the internal viscosity of the material. So that numerically the dissipation constant of a solid is given by

$$Q = \frac{E}{\omega\ \mu}.$$

The viscosity μ varies approximately inversely as the frequency, and since E is

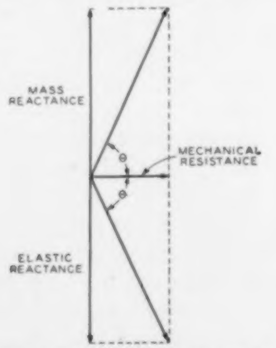


FIG. 2. THE VECTOR DIAGRAM FOR A BAR VIBRATING AT RESONANCE ALWAYS CONTAINS A RESISTANCE VECTOR. THE PHASE ANGLE θ OF EITHER OF THE IMPEDANCE VECTORS WILL THEN BE LESS THAN 90°, AND SINCE $Q = \tan \theta$, THE DISSIPATION CONSTANT WILL ALWAYS BE FINITE.

found to be independent of frequency, the quantity Q is a constant as far as frequency is concerned. From a physical standpoint, the magnitude of Q then is a measure of the relative magnitudes of reactance and resistance present in a given sample of the material. Thus far no material free from mechanical resistance has been found. The determination of Q does not necessitate a direct knowledge of E and μ, however. These two quantities may be calculated from easily made measurements of response at certain frequencies, as shown in Fig. 2. When the amplitude at resonant frequency f, and the frequencies f_1, f_2 at which the amplitude is $1/\sqrt{2}$ times this value are known, we have the relationship

$$Q = \frac{f}{f_2 - f_1}.$$

In the simplest case, then, the dissipation constant can be obtained directly from three measured frequencies.

Considerable work has been done in these laboratories on several phases of the subject. In Fig. 4 is shown H. C. Rorden with the apparatus used for measuring internal dissipation of solids. The slender bar to be measured, shown in the center of the picture, is supported at its middle on a rigid mounting. A magnetic receiver structure is placed near each end. An oscillator connected to one of the receivers is tuned to the resonance frequency of the bar. The resulting longitudinal vibration of the bar induces a voltage in the second receiver, which is amplified, rectified and read on a meter. The tuning of the driving current is controlled by the precision condenser on Mr. Rorden's right, and the response of the bar is read on the meter beside it.

A list of typical solid materials reveals a very large range of values for the dissipation constant. A minimum of 30 for the Q of a hard piece of lead and a maximum of 50,000 for a soft piece of aluminum are not unusual. It is neces-

sary to state whether a given material is hard or soft because when lead with a Q of 30 is softened by annealing, the Q may be increased to a value of 200. Similarly, when a soft piece of aluminum with a Q of 50,000 is severely cold-worked by rolling or bending, the Q may be reduced to 8,000. Again, of two bars of steel identical in size, shape and composition, but one of them hardened by heating and quenching and the other softened by careful annealing, one finds upon tapping their ends that it is the soft one which rings the longest. The dissipation constants of most solids are found between the two extremes of lead and aluminum just mentioned. The table below gives values of Q for most of the solids investigated thus far. The figures indicate orders of magnitude rather than specific values, because, as pointed out above, considerable variation is possible for a given material under various conditions of internal strain.

Quartz (fused)	over 100,000	Copper	3,000
Aluminum	50,000	Nickel	2,000
Brass	40,000	Zinc	1,900
Duralumin	30,000	Glass	1,600
Iron	20,000	Carbon (graphite)	900
Permalloy (80% nickel)	9,000	Tin	800
Silver	6,000	Hard rubber	50
		Lead	30

It is interesting to note that the order in the list bears apparently no relation to other physical properties. Thus we have two of the softest metals occupying positions near opposite extremes of the table. Two solids like zinc and glass, on the other hand, differ greatly in hardness and in electrical resistivity; still their mechanical dissipation constants are nearly the same. Again, carbon and tin are almost alike from the standpoint of mechanical dissipation; yet if we should arrange the list according to melting points, we should find the one at the head and the other at the bottom of the list. But the apparent non-correspondence with the more well-known physical properties and the extremely large range of values of dissipation constants might furnish a means for investigating hitherto unsolved problems involving different kinds of solids and different states of internal strain or structure for a given solid.

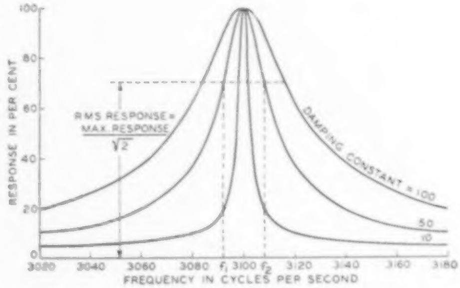


FIG. 3. TYPICAL FREQUENCY-RESPONSE CURVES FOR RESONANT BODIES. THE GENERAL EXPRESSION IS

$$Q = \frac{f}{\left[\frac{r}{\sqrt{1-r^2}}\right](f_2 - f_1)}$$

WHERE f_1 AND f_2 ARE TWO FREQUENCIES AT WHICH THE RESPONSE IS THE SAME FRACTION r OF THE MAXIMUM RESPONSE R. THE EXPRESSION BECOMES UNITY WHEN $r = \frac{R}{\sqrt{2}}$.

FIG. 4. APPARATUS USED FOR THE MEASUREMENT OF INTERNAL DISSIPATION OF SOLIDS.

VENERABLE BEDE,

BORN IN THE BISHOPRIC OF DURHAM, ENGLAND, IN A.D. 672 OR 673; DIED ON MAY 26, A.D. 735; BURIED IN THE CHURCH OF THE ABBEY OF ST. PAUL, AT JARROW, IN THE PALATINE OF DURHAM.

THE PROGRESS OF SCIENCE

BAEDA VENERABILIS (672–735)

TWELVE centuries have passed since Baeda (Beda, Bede), one of the first of the British scholars of the Middle Ages, completed his work at Jarrow. Four years before his death, which probably occurred in 735, he added a note to his Magnum Opus, the "Historia Ecclesiastica gentis Anglorum," setting forth in brief the major events of a life passed in the monasteries of St. Peter in Wearmouth and St. Paul in Jarrow. The note reads in part: "I was born in the territory of the said monastery, and at the age of seven I was, by the care of my relations, given to the reverend Abbott Benedict—to be educated. From that time I have spent the whole of my life within this monastery devoting all my pains to the study of the scriptures; and amid the observance of monastic discipline, and the daily charge of singing in the church, it has ever been my delight to learn or to teach or to write"—*semper aut discere aut docere aut scribere dulce habui*. What more noble epitaph for a scholar!

Since the current year marks the twelve-hundredth anniversary of the death of Baeda, it is fitting to recall in a scientific journal his contributions to science, even though they were of minor importance when compared with the "Ecclesiastical History," which he completed in 731. In this field he wrote upon physical sciences ("De Natura Rerum,") his material being chiefly gathered from such writers as Isidore of Seville who, in turn, had depended largely upon various earlier Latin writers. This is seen in his treatment of such subjects as the rainbow, volcanoes, thunder and the salt in the seas. He also ventured upon the vexed question of the church calendar ("De Temporum Ratione"), chronology, finger reckoning ("Tractatus de computo, vel loquela per gestum digitorum"), fractions ("De ratione unciarum") and the difficult subject of computation in the age of Roman numerals and of calculation by counters.

It was he who introduced into England the measuring of time from the birth of Christ, attributing it to Dionysius, who had announced his system in Rome beginning with March 25, 527, although it appeared in papal documents somewhat earlier.

The representing of numbers by means of fingers has a long history, as indeed has the computation by similar devices. Even at the present time in certain parts of the world people multiply 8 by 6 by leaving three fingers up on the left hand to represent $5+3$, the rest (2) being closed; they also leave 1 finger upon the right hand, the rest (4) being closed. Then they add the upright fingers ($3+1=4$) and multiply those which are closed ($2\times4=8$), thus obtaining 48. The process requires the learning of multiplication facts only to 4's.

The contribution made by Baeda was to the representing by the fingers of larger numbers than were commonly used in the period in which he lived. A manuscript copy of his works, with illustrations of the finger arrangements, is now in the Biblioteca National at Madrid, dated about 1140. Another description of these symbols is in the "Codex Alcobatiensis" in the same library. Of the several early printed illustrations of finger numerals the one in Pacioli's "Sũma de Arithmetica Geometria Proportioni & Proportionalita" (Venice, 1494) and a similar one in the "Abacvs" of Johannes Aventinus (Nürnberg, 1522) are the best. The latter writer, in his title page, pays tribute to Baeda in these words: *Abacvs atqve vetvstissima, vetervm latinorum per digitos manasqz numerandi* (*qui-*

netiam loquendi)cosuetudo, Ex beda cũ picturis & imaginibus. . . . From the 1532 edition).

A further contribution to arithmetic is the one on the study of fractions. This appears in his brief essay "De ratione vnciarvm," referring to the Roman submultiples of the *as* into *unciae*, practically the scale of twelve applied to mensuration (linear and monetary). It is particularly valuable since there is given (in the 1525 edition, Venice, which I am here consulting) the symbols for such measures as the "Deunx, vel Dextans," often puzzling to the beginner in the study of medieval documents. This edition contains also extracts from the works of M. Valerianus Probus, Petrus Diaconus and Demetrius Alabaldus—all germane to his own subject.

A man of such repute as a scholar and of prominence in the church naturally attracted the attention of the faithful in all parts of Britain. So great was his renown, even after his death, that his tomb at Jarrow became a shrine for pilgrims, urged by the belief that his relics could perform miracles in this world and could relieve souls in purgatory. Especially on the anniversaries of his death did priests and people assemble at his place of burial to watch and pray and chant the services for which he had led the singing in his monastery for many years.

In the ninth century Alcuin of York urged the monks of Northumbria to follow the path laid out by Baeda and not to forget the praise which he had received from men and the glorious reward from God. It was because of his great repute and the miracles reported by visitors to his shrine that his grave was later rifled for the purpose of carrying relics to various churches. In the eleventh century one Elfred (Alfred), a priest in the cathedral at Durham, announced himself as commissioned by heaven to collect bones from the graves of saints and expose them to the faithful. In this way it is asserted that he stole the bones of Baeda from Jarrow and carried them to Durham. The story is too long for repetition here, but to-day the traveler may see in the Galilee chapel of the noble cathedral of that city a slab bearing the inscription, "Hâc sunt in fossâ Bædæ venerabilis ossa."

As to the term "Venerable," this can hardly have been given him on account of his great age, since he died at or about the age of sixty-three. It is more probable that it was a title used somewhat as in the case of archdeacons in the Church of England to-day and for another purpose in the Church of Rome.

The portrait accompanying this sketch is from an engraving of the eighteenth or the nineteenth century and can hardly lay claim to any antiquity. In those professing to be authentic, however, there is a general resemblance which suggests an earlier drawing from which all were taken.

For those who care to follow more at length the contributions of Bede to science, reference may be made to his collected works, edited by J. A. Giles (twelve volumes, 1843–44), and the essay prefixed to his "Historia Ecclesiastica" by Charles Plummer (Oxford, 1896).

Baeda's works were published several times in the sixteenth century as in Basel (1521, 1563), the 1563 edition appearing in eight volumes bound in four. The first of these volumes contains "De Arithmeticis nvmeris liber," chiefly devoted to an extensive table of products; "De Arithmeticis proportionibvs"; "De ratione calcvli," dealing with Roman money; "De loqvela per gestvm digitorvm, et temporvm ratione"; "De ratione vnciarvm" and a treatise on the calendar with a description on the astrolabe. There was also an edition by Noviomagus (Cologne, 1537). The "De natura Rerum" was published at Basel in 1529.

DAVID EUGENE SMITH

THE TEMPLETON CROCKER PACIFIC EXPEDITION

A SYSTEMATIC investigation of the birds of the Pacific Ocean and its islands has been carried on by the American Museum of Natural History ever since 1920, when the Whitney South Sea Expedition began its fifteen-year survey of that vast area. Recently a new section, known as the Whitney Wing, has been added to the museum, and into it are being moved the study collections and offices of the Department of Birds.

Two floors in the Whitney Wing are to be devoted to exhibition, and on the second floor there will be a series of eighteen faunal habitat groups, showing scenes on a carefully selected series of islands from the American coast to New Guinea. The preparation of such exhibits involves the collecting of vegetation, samples of soil and rocks, and the sketching of landscapes, all from the exact locality to be reproduced. So when Templeton Crocker of San Francisco offered to place his elegant schooner yacht *Zaca* at the services of the American Museum for a scientific cruise in the Pacific, he provided the ideal opportunity to secure the materials needed for some of these bird groups.

The sites chosen for the first four groups were in the Marquesas Islands, the Tuamotu Archipelago, the Bird Islands of Peru and the Galápagos Islands. So widely spaced are these localities that many other islands could be visited along the way.

The Museum's Department of Anthropology has also been conducting a survey in the Pacific—that of its human inhabitants, especially the Polynesians. Dr. Harry L. Shapiro, associate curator of physical anthropology, who had already made five visits to Oceania, was therefore invited by Mr. Crocker to join the party, so as to continue his researches in physical anthropology.

For the ornithological work Dr. Frank M. Chapman designated Francis L. Jaques, staff associate in preparation and exhibition, and myself. Mr. Crocker, leader of the expedition, engaged Maurice Willows to assist in entomological collecting, and Dr. George P. Lyman as ship's surgeon.

Sailing from San Francisco on September 15, 1934, the *Zaca* called at the port of Los Angeles and then set out for the island of Nuku Hiva in the Marquesas. On October 6 she anchored in Taio Hae Bay. After a preliminary survey, we chose for the bird group a spot on a mountain ridge overlooking Taipi Bay, made famous by Herman Melville's "Typee." The foreground will contain Hibiscus trees and ferns, with fruit pigeons, warblers and swifts, as well as white terns and noddies which come up from the sea to breed.

After Nuku Hiva, brief visits were made to the islands of Hiva Oa and Fatu Hiva; and the *Zaca* proceeded to the Tuamotu Archipelago. At Tatakoto and Hao Islands Dr. Shapiro carried on anthropological investigations, and at Hao, a rather large atoll, studies were made for the second bird group. The scene here will be a curving coral reef with palms and low trees, blue water and breakers offshore. The birds are mainly sea birds, which find safe nesting grounds on uninhabited sections of the atoll.

In November a visit was made to Tahiti, where blood-group studies were made of the native population. Next, a call was made to the Austral Islands, and Dr. Shapiro pursued his studies on Rimitara, Rurutu and Raivavae. Still more interesting was the outlying island of Rapa, remarkable for the richness of its vegetation, where the natives live under more primitive conditions than in the Austral group.

A supply of fuel had been shipped in advance to Mangareva, one of the Gambier Islands. Thence the *Zaca* set out

VIEW DOWN ON TAIO HAE BAY, NUKU HIVA, MARQUESAS.

for Pitcairn Island, which was sighted on December 22. It would be difficult to find a more hospitable folk than the couple of hundred Pitcairn inhabitants. My own impression is that of a colony of civilized white people making a living on a tiny island they love, with export trade all but impossible. As a classic example in human genetics they are of prime interest to the anthropologist, and Dr. Shapiro made the very best of his great opportunity. Dr. Lyman helped repay the kindness of the islanders by giving medical or surgical care to all who needed it.

The Pitcairn people bade us a fond

PITCAIRN ISLAND, FROM 2¼ MILES TO THE EAST.

farewell from their huge surf boats on New Year's Day, 1935, and we sailed for Ducie Island, an uninhabited atoll. Our next objective was Easter Island, justly one of the most celebrated islands in the Pacific.

This easternmost outlier of Polynesia is a little below the Tropic of Capricorn, and almost due south of Salt Lake City. The huge stone images, carved and erected by a primitive Polynesian population, aroused our warm admiration. A plaster mold was made of a typical example. The native population, still numbering some 440 souls, was studied by Dr. Shapiro. There are, unfortunately, no indigenous land birds, and, except around three rocky islets on the southwest, sea birds are not abundant.

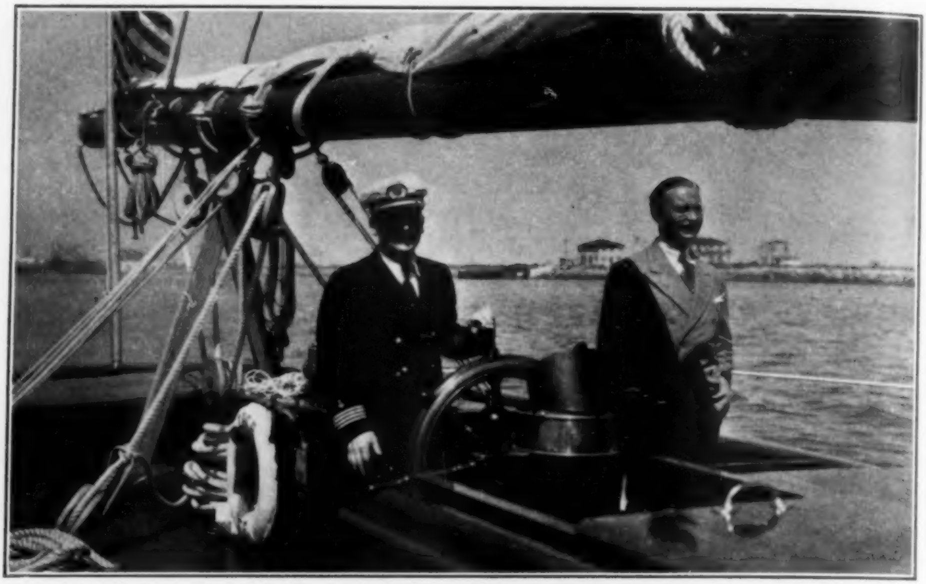
After six days at Easter Island, we sailed on January 19 for Valparaiso. On the way a day was given to each of the Juan Fernández Islands. Now that Dr. Shapiro's work was completed, he decided to leave us and return direct from Valparaiso to New York.

The *Zaca* sailed again on February 12, touched at Coquimbo and then left the coast to visit the remote volcanic islands of San Felix and San Ambrosio. The birds of this small uninhabited group were of special interest to Dr. Robert Cushman Murphy. We collected the seven species of marine birds which seem regularly to frequent it, and turned back toward the coast of Peru.

BREAKFAST TIME AT TERIN BAY.

Now the bird group work began again. A spot was chosen on South Chincha Island to show the vast numbers of cormorants, boobies and pelicans which nest on these famous guano islands. Some years ago Mr. Jaques did preliminary work in this region, which has been so exhaustively studied by Dr. Murphy. So a few days sufficed to complete our work, which had the special authorization of the Peruvian government.

Next came a brief visit to Callao and Lima, then the voyage to the Galápagos.



ON THE *ZACA*, OFF CALIFORNIA.
RIGHT; TEMPLETON CROCKER. LEFT; CAPTAIN ALFRED PEDERSEN OF THE ZACA.

A visit to Tagus Cove on Albemarle Island showed that while it is an excellent spot for the study of some of the sea birds, and especially the flightless cormorant, it was not a suitable locality for our group. It is intended to show the indigenous land birds of the archipelago, the dove, hawk, cuckoo, mocking bird and

ONE OF THE SURF-BOATS AT PITCAIRN ISLAND.

finches of the *Geospiza* group, rather than the marine species.

So the *Zaca* took us around the north end of Albemarle, to James Island, and then on to the more central island of Indefatigable. Here land birds occur in some abundance, and our studies were made at Conway Bay during a period of nearly two weeks. This was in March, a hot, rainy month, when the vegetation is at its best, and many of the smaller birds are nesting. Before we left the Galápagos for Panama, a morning was spent amid the nesting frigate-birds, boobies and fork-tailed gulls on Tower Island.

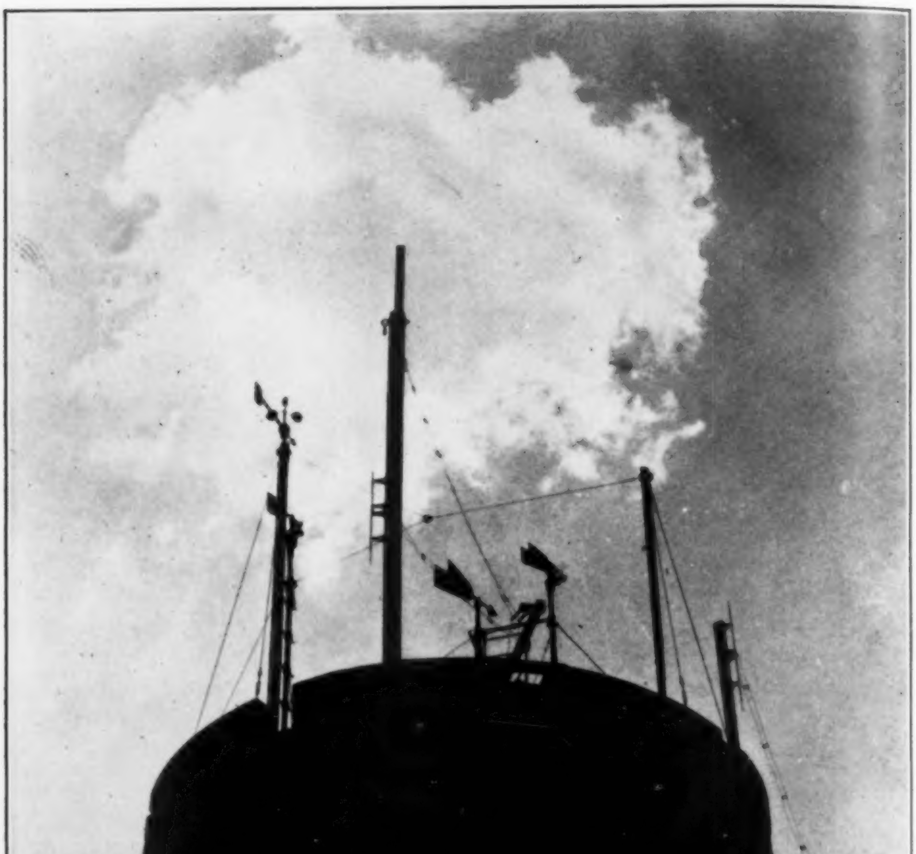
On the dock at Balboa on April 1 stood Dr. Frank M. Chapman, who had come over from his "Tropical Air Castle" to welcome Mr. Crocker and assistants in the American Museum.

The results of our expedition include not only the anthropological data, materials for the bird groups and the numerous and admirable paintings by Mr. Jaques, but also photographs numbering over 1,500 and very many specimens gathered here and there along the way. A rich collection of marine invertebrates, fishes and algae was made personally by Mr. Crocker. His series of flying fishes, large and small, is especially remarkable. He and Mr. Willows secured numbers of insects from many of the islands. Then there are also the anthropometric series, blood-groupings and skeletal material from many of the islands, and 435 dried plants.

These collections are being divided between the American Museum, the California Academy and the New York Botanical Garden. I scarcely need add how greatly indebted we feel to Templeton Crocker, whose distinguished patronage has thus promoted scientific research in the Pacific, as well as the educational purposes of the new Whitney Hall.

James P. Chapin

VIEW OF THE BAY OF THE VIRGINS.
Fatu Hiva Island, Marquesas, from a nearby hill with Zaca at anchor.



THE BLUE HILL OBSERVATORY TOWER

SHOWING THE $2\frac{1}{2}$- AND 5-METER RADIO ANTENNAE.

SHORT-WAVE RADIO EXPERIMENTS AT THE BLUE HILL OBSERVATORY OF HARVARD COLLEGE

WHEN Hertz began his radio experiments he chose to work with the very short waves, but as these waves appeared to have no important commercial application, later studies were directed to developing the long waves, and in general the history of radio shows progress from the longer waves, first used by Marconi to span the Atlantic, to waves of shorter and shorter length, until to-day waves of a few centimeters in length have been produced in the laboratory, linking together the short-wave portion of the radio spectrum and the long-wave end of the light spectrum. In the early days of radio communication waves as long as eighteen thousand meters were used to cover great distances and those shorter than two hundred meters were considered valueless. As experience was gained with these shorter waves, however, it appeared that with very low power it was possible to signal over thousands of miles, due to the fact that these signals were reflected back to the surface of the earth by the high ionized region known as the Kennelly-Heaviside layer. As the length of radio waves is decreased to about seven meters they cease to be reflected back by this layer, and it was assumed that there was nothing to cause them to bend around the earth, with the result that they would be lost in space and not heard at distant points. Waves shorter than seven meters would then be limited to signaling between points which were intervisible, and no variations in the signal strength would be expected.

Although short waves have been made experimentally for some years it is only recently that systematic study of the ultra-high frequencies has been carried on. When the Mt. Washington Observatory was established in 1932, on the summit of Mt. Washington, in New Hampshire, it was decided to make use of five-meter radio waves for communication purposes. It soon proved that these signals could be heard at relatively great distances, and during the winter equipment was installed at the Blue Hill Observatory with which communication is maintained with the mountain at practically all times. By means of a tone signal which is sent out at regular intervals from Blue Hill it is possible to measure the intensity of the signal at distant points and, beginning in 1932, such measurements were made at Mt. Washington, and later at other points. These distant points are below the direct optical line of sight, and Mt. Washington is one hundred and forty-two miles from Blue Hill. It was thus clear that such radio signals were able to bend in some manner and it was also found that, instead of remaining at a steady level at all times, the signals rose and fell, showing certain regular periodicities. Later on, automatic photographic recording equipment was installed at several of the observing points so that more continuous records could be kept. As a result of these measurements a well-defined diurnal change in signal was found, with a high level during the night, maxima in the morning and evening and a minimum near noon, also a probable seasonal variation, with best transmission during the summer. Certain periods are also characterized by a good level of signals, while at other times the signals may be poor for days at a time.

Atmospheric conditions during such periods of transmission suggest that exceptionally good reception of short-wave signals is associated with temperature inversions in the atmosphere. The improvement in reception may be due to the increased refraction of radio waves in the atmosphere under these conditions. This refraction of radio waves, when added to the normal bending which the waves undergo by diffraction in pass-

BLUE HILL OBSERVATORY RADIO EQUIPMENT.

THE $2\frac{1}{2}$-METER TRANSMITTER IS AT THE LEFT; THE 5-METER TRANSMITTER AND RECEIVER CAN BE SEEN TO THE RIGHT; WITH THE AUTHOR IN THE FOREGROUND.

ing over hills, may well account for the occasional long-distance reception of ultra-high frequency signals.

During the past year experiments have been begun with even shorter waves, and at present communication is also possible over the long-distance circuits from Blue Hill on a wave of two and a half meters. These signals show even greater variations than those experienced on five meters, but are still useful for communication. Signals have been received over a few miles on a wave of one and a quarter meters. A fascinating field of research is opened up in these new regions, both in the systematic study of their behavior and also in the development of the technique of generating them.

Experience with the short waves has indicated some interesting new applications of them, among which is their use for sending back radio signals from free meteorological sounding balloons. An ascent into the stratosphere in a manned balloon is an expensive and dangerous undertaking. It is now proposed to make more general use of ultra-high frequency radio to send us back indication of weather conditions in this region, otherwise so inaccessible. Very small and light transmitters are being developed as well as directive receiving systems to aid in plotting the position of the balloon. Thus radio may play an important new part in the future of meteorological study.

ARTHUR EDWARD BENT
BLUE HILL OBSERVATORY,
HARVARD UNIVERSITY